# EXPERIMENTAL ENQUIRY

CONCERNING THE

## Natural Powers of Wind and Water

TO TURN

# MILLS,

AND OTHER

# MACHINES,

DEPENDING ON A CIRCULAR MOTION.

AND AN

# EXPERIMENTAL EXAMINATION

*Of the Quantity and Proportion of*

# MECHANIC POWER

Necessary to be employed in giving different degrees of Velocity to

*HEAVY BODIES FROM A STATE OF REST.*

ALSO

## NEW FUNDAMENTAL EXPERIMENTS

UPON THE

# *COLLISION OF BODIES,*

WITH FIVE PLATES OF MACHINES.

BY THE LATE MR. JOHN SMEATON, F. R. S.

*THE THIRD EDITION.*

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# *ADVERTISEMENT.*

*THE subjects of the following sheets, as branches of practical mechanics, are interesting; and the manner in which they are treated render them important.*

*Previous to these experiments, the superior power of overshot water wheels was not only doubted, but some authors of considerable celebrity had given a preference to undershot wheels.—The experiments made, and here fully explained by Mr.* SMEATON *(who, as a practical mechanic, must ever be rated in the first class), shew the fallacy of former conclusions, and the great superior power of overshot wheels: the lapse of time since the publication of these principles, has confirmed their accuracy, and has established a durable reputation for their author.*

*These Essays, published originally in the Philosophical Transactions (the parts of which are now become scarce, and of high price on their account), are now offered to the public at a moderate purchase; to render them*

*them accessible to every ingenious enquirer, must tend to spread the very useful information they contain. To a superior mind nothing can be more grateful, than imparting to others the advantages of that superiority, which bountiful Heaven has bestowed on enlightened Genius.*

# CONTENTS.


Page

OF UNDERSHOT WATER WHEELS - - - 2

I. That the virtual or effective Head being the same, the Effect will be nearly as the Quantity of Water expended - - - - - - - 15

II. That the Expence of Water being the same, the Effect will be nearly as the Height of the virtual or effective Head - - - - - 17

III. That the Quantity of Water expended being the same, the Effect is nearly as the Square of its Velocity - - - - - - - - - - 19

IV. The Aperture being the same, the Effect will be nearly as the Cube of the Velocity of the Water - - - - - - - - - - - - 21

OF OVERSHOT WHEELS - - - - - - - - 25

I. Of the Ratio between the Power and Effect of Overshot Wheels - - - - - - - - - 29

II. Of the proper Height of the Wheel in Proportion to the whole Descent - - - - - 31


III. Of

Page

III. Of the Velocity of the Circumference of the Wheel, in order to produce the greatest Effect - - - - - - - - - - - - - 32
IV. Of the Load for an Overshot Wheel, in order that it may produce a Maximum - - - - 34
V. Of the greatest possible Velocity of an Overshot Wheel - - - - - - - - - - - 34
VI. Of the greatest Load that an Overshot Wheel can overcome - - - - - - - - - - 35

ON THE CONSTRUCTION AND EFFECT OF WINDMILL SAILS - - - - - - - - - 38
I. Of the best Form and Position of Windmill Sails - - - - - - - - - - - - - 43
II. Of the Ratio between the Velocity of Windmill Sails unloaded, and their Velocity when loaded to a Maximum - - - - - - - - - 50
III. Of the Ratio between the greatest Load the Sails will bear without stopping, &c. - - 50
IV. Of the Effects of Sails, according to the different Velocity of the Wind - - - - - 51
V. Of the Effects of Sails of different Magnitudes, the Structure and Position being similar, and the Velocity of the Wind the same - - - - - - - - - - - - - - 55
VI. Of the Velocity of the Extremities of Windmill Sails, in respect of the Velocity of the Wind - - - - - - - - - - - - - - - 57

VII. Of

Page

VII. Of the absolute Effect, produced by a given Velocity of the Wind, upon Sails of a given Magnitude and Construction - - - - - 61

VIII. Of HORIZONTAL WINDMILLS and Water Wheels, with oblique Vanes - - - - - 63

General Proposition - - - - - - - - - - 66

EXPERIMENTAL EXAMINATION OF THE QUANTITY OF MECHANIC POWER NECESSARY TO MOVE HEAVY BODIES, FROM A STATE OF REST - - - - - - - - - - - - - - 71

EXPERIMENTS UPON THE COLLISION OF BODIES - - - - - - - - - - - - - 95

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*Directions to the Binder.*

TABLE I. is to face Page 15.
TABLE III. is to face Page 43.
The five PLATES to be put at the End.

AN

# EXPERIMENTAL ENQUIRY

CONCERNING THE

## *NATURAL POWERS*

OF

# WATER AND WIND

## To turn *Mills* and other *Machines*, depending on

## A CIRCULAR MOTION;

*Read before the Royal Society, May* 3, & 10, 1759.

WHAT I have to communicate on this subject was originally deduced from experiments made on working models, which I look upon as the best means of obtaining the outlines in mechanical enquiries. But in this case it is very necessary to distinguish the circumstances in which a model differs from a machine in large; otherwise a model is more apt to lead us from the truth than towards it. Hence the common observation, that a thing may do very well in a model that will not answer in large. And, indeed, though the utmost circumspection be used in this way, the best structure of machines cannot be fully ascertained, but by making trials with them, when made of their proper size. It is for this reason that though the models re-

ferred to, and the greatest part of the following experiments, were made in the year 1752 and 1753, yet I deferred offering them to the Society, until I had an opportunity of putting the deductions made therefrom in real practice, in a variety of cases, and for various purposes; so as to be able to assure the Society that I have found them to answer.

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# PART I.

## *Concerning Undershot Water Wheels.*

PLATE I. Fig. 1. is a perspective view of the machine for experiments on water-wheels; wherein

A B C D is the lower cistern, or magazine, for receiving the water, after it has quitted the wheel; and for supplying

D E the upper cistern or head; wherein the water being raised to any height required by a pump, that height is shewn by

F G, a small rod, divided into inches and parts; with a float at the bottom, to move the rod up and down, as the surface of the water rises and falls.

H I is a rod by which the sluice is drawn, and stopt at any height required, by means of

K a pin, or peg, which fits several holes, placed in the manner of a diagonal scale, upon the face of the rod H I.

G L is the upper part of the rod of the pump, for drawing the water out of the lower cistern, in order to raise and keep

keep up the surface thereof at its desired height, in the head D E; thereby to supply the water expended by the aperture of the sluice.

M M is the arch and handle for working the pump, which is limited in its stroke by

N, a piece for stopping the handle from raising the piston too high; that also being prevented from going too low, by meeting the bottom of the barrel.

O is the cylinder, upon which a cord winds, and which, being conducted over the pullies P and Q, raises

R, the scale, into which the weights are put, for trying the power of the water.

S T, the two standards, which support the wheel, are made to slide up and down, in order to adjust the wheel, as near as possible, to the floor of the conduit.

W the beam which supports the scale and pullies; this is represented as but little higher than the machine, for the sake of bringing the figure into a moderate compass, but in reality is placed 15 or 16 feet higher than the wheel.

PLATE II. Fig. 2. is a section of the same machine, wherein the same parts are marked with the same letters as in Fig. 1. Besides which

X X is the pump barrel, being 5 inches diameter, and 11 inches long.

Y is the piston; and

Z the fixed valve.

G V is a cylinder of wood, fixed upon the pump-rod, and reaches above the surface of the water: this piece of wood being of such a thickness, that its section is half the area of that of the pump-barrel, will cause the surface of water to rise in the head, as much while the piston is descending, as while it is rising: and will thereby keep the guage-rod F G more equally to its height.—*Note*, The arch and handle M M is here represented on a different side to what it is shewn in the preceding figures, in order that its dimensions may the better appear.

*a a* shews one of the two wires which serve as directors to the float, in order that the guage-rod F G may be kept perpendicular; for the same purpose also serves *w*, a piece of wood with a hole to receive the guage-rod, and keep it upright.

*b* is the aperture of the sluice.

*c c* a kant-board, for throwing the water more directly down the opening *c d*, into the lower cistern; and

*c e* is a sloping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3. represents one end of the main axis; with a section of the moveable cylinder, marked O in the preceding figures.

A B C D is the end of the axis; whereof the parts

B and D are covered with ferrules or hoops of brass.

E is a cylinder of metal; whereof the part marked

F is the pivot or gudgeon

*c c* is

*c c* is the section of an hollow cylinder of wood, the diameter of the interior part being somewhat larger than the cylindrical ferrule B.

*a a* is the section of a ferrule of brass, driven into the end of the hollow cylinder, and which is adjusted to that marked B, so as to slide freely thereupon, but with as little shake as possible.

*b b*, *d d*, *g g*, represent the section of a brass ferrule, plate, and socket, fixed upon the other end of the hollow cylinder; the socket *d d* being adjusted to slide freely upon the cylinder E, in the same manner as the ferrule *a a* slides upon the cylinder B: the outer end of the socket at

*g g* is formed into a sort of button; by pushing whereof, the hollow cylinder will move backwards and forwards, or turn round at pleasure upon the cylindrical parts of the axis B and E.

*e e*, *i i*, *o o*, represent the section of a brass ferrule, also fixed upon the hollow cylinder: the edge of this ferrule

*e e* is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

*o o* is cut in the manner of a ratchet.

Of consèquence, when the plate *b d d b* is pushed close to the ferrule D, the teeth of the ferrule *e e* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *g g*, the hollow cylinder is thereby disengaged from the pin G, and ceases

turning. *Note*, The weight in the scale is prevented from running back, by a catch that plays in and lays hold of the ratchet *o o*.

By this means the hollow cylinder upon which the cord winds and raises the weight, is put in action and discharged therefrom instantaneously, while the wheel is in motion: for, without some contrivance of this kind, it would not be easy to make this sort of experiments with any tolerable degree of exactness.

The use of the apparatus now described will be rendered more intelligible, by giving a general idea of what I had in view; but as I shall be obliged to make use of a term which has heretofore been the cause of disputation, I think it necessary to assign the sense in which I would be understood to use it; and in which I apprehend it is used by practical *Mechanicks*.

The word *Power*, as used in practical mechanicks, I apprehend to signify the exertion of strength, gravitation, impulse, or pressure, so as to produce motion: and by means of strength, gravitation, impulse, or pressure, compounded with motion, to be capable of producing an effect: and that no effect is properly mechanical, but what requires such a kind of power to produce it.

The raising of a weight, relative to the height to which it can be raised in a given time, is the most proper measure of power; or, in other words, if the weight raised is multiplied by the height to which it can be raised in a given time, the product is the measure of the power raising it; and consequently, all those powers are equal, whose products, made by such multiplication, are equal: for if a power can raise twice the weight to the same height; or the same weight to twice the height, in the same time that another power can, the first power is double the

second:

second; and if a power can raise half the weight to double the height; or double the weight to half the height, in the same time that another can, those two powers are equal. But *note*, all this is to be understood in case of slow or equable motion of the body raised; for in quick, accelerated, or retarded motions, the *vis inertiæ* of the matter moved will make a variation.

In comparing the effects produced by water-wheels with the powers producing them; or, in other words, to know what part of the original power is necessarily lost in the application, we must previously know how much of the power is spent in overcoming the friction of the machinery, and the resistance of the air; also what is the real velocity of the water at the instant that it strikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the instant that it strikes the wheel, given, the height of head productive of such velocity can be deduced, from acknowledged and experimented principles of hydrostatics: so that by multiplying the quantity, or weight of water, really expended in a given time, by the height of a head so obtained: which must be considered as the height from which that weight of water had descended in that given time; we shall have a product, equal to the original power of the water, and clear of all uncertainty that would arise from the friction of the water, in passing small apertures; and from all doubts, arising from the different measure of spouting waters, assigned by different authors. On the other hand, the sum of the weights raised by the action of this water, and of the weight required to overcome the friction and resistance of the machine, multiplied by the height to which the weight can be raised in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the *power* to the *effect*: so that by loading the wheel with different weights successively, we shall be

be able to determine at what particular load, and velocity of the wheel, the effect is a *maximum*.

The manner of finding the real velocity of the water, at the instant of its striking the wheel; the manner of finding the value of the friction, resistance, &c. in any given case; and the manner of finding the real expence of water, so far as concerns the following experiments, without having recourse to theory; being matters upon which the following determinations depend, it will be necessary to explain them.

## *To determine the Velocity of the Water striking the Wheel.*

It has already been mentioned, in the reference to the figures, that weights are raised by a cord winding round a cylindrical part of the axis. First, then, let the wheel be put in motion by the water, but without any weights in the scale; and let the number of turns in a minute be 60: now it is evident, that was the wheel free from friction and resistance, that 60 times the circumference of the wheel would be the space through which the water would have moved in a minute; with that velocity wherewith it struck the wheel: but the wheel being incumbered by friction and resistance, and yet moving 60 turns in a minute, it is plain that the velocity of the water must have been greater than 60 circumferences before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the usual way, and put a weight in the scale; the weight so disposed (which may be called the *counter-weight*) will endeavour to assist the wheel in turning the same way, as it would have been turned by the water: put therefore as much weight into the scale as, without any water, will cause it to turn somewhat faster than at the rate of 60 turns in a minute; suppose 63; let it now be tried again by the water, assisted by the weight;

the

the wheel therefore will now make more than 60 turns; suppose 64: hence we conclude the water still exerts some power in giving motion to the wheel. Let the weight be again increased, so as to make $64\frac{1}{2}$ turns in a minute without water: let it once more be tried with water as before; and suppose it now to make the same number of turns with water as without, *viz.* $64\frac{1}{2}$: hence it is evident, that in this case the wheel makes the same number of turns in a minute, as it would do if the wheel had no friction or resistance at all; because the weight is equivalent thereto; for was it too little the water would accelerate the wheel beyond the weight; and if too great, retard it; so that the water now becomes a *regulator* of the wheel's motion; and the velocity of its circumference becomes a measure of the velocity of the water.

In like manner, in seeking the greatest product, or *maximum*, of effect; having found by trials what weight gives the greatest product, by simply multiplying the weight in the scale by the number of turns of the wheel, find what weight in the scale, when the cord is on the contrary side of the cylinder, will cause the wheel to make the same number of turns the same way, without water; it is evident that this weight will be nearly equal to all friction and resistance taken together; and consequently, that the weight *in* the scale, with twice * the weight *of* the scale, added to the back or counter-weight, will be equal to the weight that could have been raised, supposing the machine had been without friction or resistance; and which multiplied by the height to which it was raised, the product will be the greatest effect of that power.

* The weight of the scale makes part of the weight both ways.

## *The Quantity of Water expended is found thus:*

The pump made use of for replenishing the head with water was so carefully made, that, no water escaping back by the leathers, it delivered the same quantity of water at every stroke, whether worked quick or slow; and as the length of the stroke was limited, consequently the value of one stroke (or, on account of more exactness, 12 strokes) was known, by the height to which the water was thereby raised in the head; which, being of a regular figure, was easily measured. The sluice, by which the water was drawn upon the wheel, was made to stop at certain heights by a peg; so that when the peg was in the same hole, the aperture for the effluent water was the same. Hence the quantity of water expended by any given head, and opening of the sluice, may be obtained: for, by observing how many strokes a minute was sufficient to keep up the surface of the water at the given height, and multiplying the number of strokes by the value of each, the water expended by any given aperture and head in a given time will be given.

These things will be further illustrated by going over the *calculus* of one set of experiments.

## *Specimen of a Set of Experiments.*

The sluice drawn to the first hole.

| | | |
|---|---|---|
| The water above the floor of the sluice | 30 | Inches. |
| Strokes of the pump in a minute - - | $39\frac{1}{2}$ | |
| The head raised by 12 strokes - - | 21 | Inches. |
| The wheel raised the empty scale, and made turns in a min. | | 80 |
| With a counter-weight of 1 lb. 8 oz. it made - - | | 85 |
| Ditto tried with water - - - - - - | | 86 |

| No. | Weight. *lb.* *oz.* | Tuns in a Min. | Product. |
|---|---|---|---|
| 1 | 4 0 | 45 | 180 |
| 2 | 5 0 | 42 | 210 |
| 3 | 6 0 | $36\frac{1}{4}$ | $217\frac{1}{2}$ |
| 4 | 7 0 | $33\frac{3}{4}$ | $236\frac{1}{4}$ |
| 5 | 8 0 | 30 | 240 maximum. |
| 6 | 9 0 | $26\frac{1}{2}$ | $238\frac{1}{2}$ |
| 7 | 10 0 | 22 | 220 |
| 8 | 11 0 | $16\frac{1}{2}$ | $181\frac{1}{2}$ |
| 9 | 12 | * ceased working. | |

Counter-weight, for 30 turns without water, 2 oz. in the scale.

*N. B.* The area of the head was 105,8 square inches.
Weight of the empty scale and pulley, 10 oz.
Circumference of the cylinder, 9 inches.
Circumference of the water-wheel, 75 ditto.

*Reduction of the above Set of Experiments.*

The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a second, equal to 107,5 inches, or 8,96 feet, which is due to a head of 15 inches †; and this we call the *virtual* or *effective* head.

The

* *N. B.* When the wheel moves so slow as not to rid the water so fast as supplied by the sluice, the accumulated water falls back upon the aperture, and the wheel immediately ceases moving.

† This is determined upon the common maxim of hydrostatics, that the velocity of spouting waters is equal to the velocity that an heavy

The area of the head being 105,8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ,579 of the ounce avoirdupoise, gives 61,26 ounces for the weight of as much water, as is contained in the head, upon 1 inch in depth, $\frac{1}{16}$ of which is 3,83 pounds; this multiplied by the depth 21 inches, gives 80,43 lb. for the value of 12 strokes; and by proportion, $39\frac{1}{2}$ (the number made in a minute) will give 264,7 lb. the weight of water expended in a minute.

Now as 264,7 lb. of water may be considered as having descended through a space of 15 inches in a minute, the product of these two numbers 3970 will express the *power* of the water to produce mechanical effects; which were as follows:

The velocity of the wheel at the *maximum*, as appears above, was 30 turns in a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the scale was hung by a pulley and double line, the weight was only raised half of this, *viz.* 135 inches.

| | | |
|---|---|---|
| The weight in the scale at the maximum | 8*lb.* | 0*oz.* |
| Weight of the scale and pulley - - | 0 | 10 |
| Counter-weight, scale, and pulley - | 0 | 12 |
| Sum of the resistance - - - - | 9 | 6 |
| or *lb.* 9,375. | | |

Now as 9,375 lb. is raised 135 inches, these two numbers being multiplied together, the product is 1266, which expresses the effect produced at a maximum: so that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3,18.

heavy body would acquire in falling from the height of the reservoir; and is proved by the rising of jets to the height of their reservoirs nearly.

But

But though this is the greatest *single* effect producible from the power mentioned, by the impulse of the water upon an undershot wheel; yet, as the whole power of the water is not exhausted thereby, this will not be the true ratio between the *power* of the water, and the *sum* of all the *effects* producible therefrom: for as the water must necessarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that some part of the power of the water must remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns a minute; and consequently its circumference moves at the rate of 3,123 feet a second, which answers to a head 1,82 inches; this being multiplied by the expence of water in a minute, *viz.* 264,7 lb. produces 481 for the power *remaining* in the water after it has passed the wheel: this being therefore deducted from the original power 3970, leaves 3489, which is that *part* of the power which is spent in producing the effect 1266; and consequently the part of the power spent in producing the effect, is to the greatest effect producible thereby as 3489 : 1266 : : 10 : 3,62, or as 11 to 4.

The *velocity of the water* striking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the *velocity of the wheel* at the *maximum* to be 30; the velocity of the water will therefore be to that of the wheel as 86 to 30; or as 10 to 3,5, or as 20 to 7.

The *load at the maximum* has been shown to be equal to 9lb. 6 oz. and that the wheel ceased moving with 12 lb. in the scale: to which if the weight of the scale is added, *viz.* 10 ounces *, the proportion will be nearly as 3 to 4 between

* The resistance of the air in this case ceases, and the friction is not added, as 12lb. in the scale was sufficient to stop the wheel after it had been in full motion; and therefore somewhat more than a counterbalance to the impulse of the water.

the

the load at the *maximum* and *that* by which the wheel is stopped.

It is somewhat remarkable, that though the velocity of the wheel in relation to the water turns out greater than $\frac{1}{3}$ of the velocity of the water, yet the impuse of the water in the case of a *maximum* is more than double of what is assigned by theory; that is, instead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It must be remembered, therefore, that in the present case, the wheel was not placed in an open river, where the natural current, after it has communicated its impulse to the float, has room on all sides to escape, as the theory supposes; but in a conduit, or race, to which the float being adapted, the water cannot otherwise escape than by moving along with the wheel. It is observable, that a wheel working in this manner, as soon as the water meets the float, receiving a sudden check, it rises up against the float, like a wave against a fixed object; insomuch that when the sheet of water is not a quarter of an inch thick before it meets the float, yet this sheet will act upon the whole surface of a float, whose height is 3 inches; and consequently was the float no higher than the thickness of the sheet of water, as the theory also supposes, a great part of the force would have been lost, by the water dashing over the float *.

In

* Since the above was written, I find that Professor Euler, in the Berlin Acts for the year 1748, in a memoir entitled *Maxims pour arranger le plus avantageusment les machines destinees a elever de l'eau par le moyen de pompes*, page 192. § 9. has the following passage; which seems to be the more remarkable, as I do not find he has given any demonstration of the principle therein contained, either from theory or experiment; or has made any use thereof in his calculations on this subject:——" Cependant dans ce cas puisque " l'eau est reflechie, & qu'elle decoule sur les aubes vers les cotés, " elle y exerce encore une force particuliere, dont l'effet de l'im-

" pulsion

# TABLE I.

| N°. | Height of water in the cistern. | Turns of the wheel unloaded. | Virtual head deduced therefrom. | Turns at the maximum. | Load at the equilibrium. | Load at the maximum. | Water expended in a minute. | Power. | Effect. | Ratio of the power and effect. | Ratio of the velocity of the water and wheel. | Ratio of the load at the equilibrium, to the load at the maximum. | Experiments. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | In. | | In. | | *lb.* *oz.* | *lb.* *oz.* | | | | | | | |
| 1 | 33 | 88 | 15,85 | 30, | 13 10 | 10 9 | 275, | 4358 | 1411 | 10:3,24 | 10:3,4 | 10:7,75 | |
| 2 | 30 | 86 | 15,0 | 30, | 12 10 | 9 6 | 264,7 | 3970 | 1266 | 10:3,2 | 10:3,5 | 10:7,4 | |
| 3 | 27 | 82 | 13,7 | 28, | 11 2 | 8 6 | 243, | 3329 | 1044 | 10:3,15 | 10:3,4 | 10:7,5 | |
| 4 | 24 | 78 | 12,3 | 27,7 | 9 10 | 7 5 | 235, | 2890 | 901,4 | 10:3,12 | 10:3,55 | 10:7,53 | At |
| 5 | 21 | 75 | 11,4 | 25,9 | 8 10 | 6 5 | 214, | 2439 | 735,7 | 10:3,02 | 10:3,45 | 10.7,32 | the |
| 6 | 18 | 70 | 9,95 | 23,5 | 6 10 | 5 5 | 199, | 1970 | 561,8 | 10:2,85 | 10:3,36 | 10:8,02 | 1st |
| 7 | 15 | 65 | 8,54 | 23,4 | 5 2 | 4 4 | 178,5 | 1524 | 442,5 | 10:2,9 | 10:3,6 | 10:8,3 | hole. |
| 8 | 12 | 60 | 7,29 | 22, | 3 10 | 3 5 | 161, | 1173 | 328, | 10:2,8 | 10:3,77 | 10:9,1 | |
| 9 | 9 | 52 | 5,47 | 19, | 2 12 | 2 8 | 134, | 733 | 213,7 | 10:2,9 | 10:3,65 | 10:9,1 | |
| 10 | 6 | 42 | 3,55 | 16, | 1 12 | 1 10 | 114, | 404,7 | 117 | 10:2,82 | 10:3,8 | 10:9,3 | |
| 11 | 24 | 84 | 14,2 | 30,75 | 13 10 | 10 14 | 342, | 4890 | 1505 | 10:3,075 | 10:3,66 | 10:7,9 | |
| 12 | 21 | 81 | 13,5 | 29, | 11 10 | 9 6 | 297, | 4009 | 1223 | 10:3,01 | 10:3,62 | 10:8,05 | |
| 13 | 18 | 72 | 10,5 | 26, | 9 10 | 8 7 | 285, | 2993 | 975 | 10:3,25 | 10:3,6 | 10:8,75 | At |
| 14 | 15 | 69 | 9,6 | 25, | 7 10 | 6 14 | 277, | 2659 | 774 | 10:2,92 | 10:3,62 | 10:9, | the |
| 15 | 12 | 63 | 8,0 | 25, | 5 10 | 4 14 | 234, | 1872 | 549 | 10:2,94 | 10:3,97 | 10:8,7 | 2d. |
| 16 | 9 | 56 | 6,37 | 23, | 4 0 | 3 13 | 201, | 1280 | 390 | 10:3,05 | 10:4,1 | 10:9,5 | |
| 17 | 6 | 46 | 4,25 | 21, | 2 8 | 2 4 | 167,5 | 712 | 212 | 10:2,98 | 10:4,55 | 10:9, | |
| 18 | 15 | 72 | 10,5 | 29, | 11 10 | 9 6 | 357, | 3748 | 1210 | 10:3,23 | 10:4,02 | 10:8,05 | |
| 19 | 12 | 66 | 8,75 | 26,75 | 8 10 | 7 6 | 330, | 2887 | 878 | 10:3,05 | 10:4,05 | 10:8,1 | The |
| 20 | 9 | 58 | 6,8 | 24,5 | 5 8 | 5 0 | 255, | 1734 | 541 | 10:3,01 | 10:4,22 | 10:9,1 | 3d. |
| 21 | 6 | 48 | 4,7 | 23,5 | 3 2 | 3 0 | 228, | 1064 | 317 | 10:2,99 | 10:4,9 | 10:9,6 | |
| 22 | 12 | 68 | 9,3 | 27, | 9 2 | 8 6 | 359, | 3338 | 1006 | 10:3,02 | 10:3,97 | 10:9,17 | |
| 23 | 9 | 58 | 6,8 | 26,25 | 6 2 | 5 13 | 332, | 2257 | 686 | 10:3,04 | 10:4,52 | 10:9,5 | 4th. |
| 24 | 6 | 48 | 4,7 | 24,5 | 3 12 | 3 8 | 262, | 1231 | 385 | 10:3,13 | 10:5,1 | 10:9,35 | |
| 25 | 9 | 60 | 7,29 | 27,3 | 6 12 | 6 6 | 355, | 2588 | 783 | 10:3,03 | 10:4,55 | 10:9,45 | 5th. |
| 26 | 6 | 50 | 5,03 | 24,6 | 4 6 | 4 1 | 307, | 1544 | 450 | 10:2,92 | 10:4,9 | 10:9,3 | |
| 27 | 6 | 50 | 5,03 | 26, | 4 15 | 4 9 | 360, | 1811 | 534 | 10:2,95 | 10:5,2 | 10:9,25 | 6th. |
| 1. | 2. | 3. | 4. | 5. | 6. | 7. | 8. | 9. | 10. | 11. | 12. | 13. | |

To face page 15.

In further confirmation of what is already delivered, I have adjoined a table, (TABLE I.) containing the result of 27 sets of experiments, made and reduced in the manner above specified. What remains of the theory of undershot wheels, will naturally follow from a comparison of the different experiments together.

## *Maxims and Observations deduced from the foregoing Table of Experiments.*

Maxim I. *That the virtual or effective head being the same, the effect will be nearly as the quantity of water expended.*

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing sets of experiments; as for

*Example* 1st, *taken from* N°. 8 *and* 25 viz.

| N°. | Virtual Head. | Water expended. | Effect. |
|---|---|---|---|
| 8 | 7,29 | 161 | 328 |
| 25 | 7,29 | 355 | 785 |

Now the heads being equal, if the effects are proportioned to the water expended, we shall have by maxim 1st, 161 : 355 : : 328 : 723; but 723 falls short of 785, as it turns out in experiment, according to N°. 25, by 62; the effect therefore of

" pulsion sera augmenté; & perienex ce jointe a la theorie a fait
" voir que dans ce cas, la force est presque double: de sorte qui'il
" faut prendre le double de le section du fil d'eau pour ce qui repond
" dans ce cas a le surface des aubes, poûrvu qu'ells soient assez
" larges pour recevoir ce supplement de force. Car si les aubes
" nétoient plus larges que le fil, on trait d'eau on ne devroit prendre
" que ne simple section, tout comme dans le premier cas, on l'aube
" toute entire est pappee par l'eau."

N°.

N°. 25, compared with N°. 8, is greater than according to the present maxim in the ratio of 14 to 13.

The foregoing example, with four similar ones, are seen at one view in the following Table.

| Examples. | N°. Tab. I. | Virtual Head. | Expence of Water. | Effect. | Comparison. | Variation. | Proportional Variation. |
|---|---|---|---|---|---|---|---|
| | | *Inch* | *b* | | | | |
| 1st | 8 | 7,29 | 161 | 328 | 161 : 328 :: 355 : 723 | 62+ | 14 : 13 |
| | 25 | 7,29 | 355 | 785 | | | |
| 2d | 13 | 10,5 | 285 | 975 | 285 : 975 :: 357 : 1221 | 11— | 121 : 122 |
| | 18 | 10,5 | 357 | 1210 | | | |
| 3d | 22 | 6,8 | 255 | 541 | 255 : 541 :: 332 : 704 | 18— | 38 : 39 |
| | 23 | 6,8 | 332 | 686 | | | |
| 4th | 21 | 4,7 | 228 | 317 | 228 : 317 :: 262 : 364 | 21+ | 18 : 17 |
| | 24 | 4,7 | 262 | 385 | | | |
| 5th | 26 | 5,03 | 307 | 450 | 307 : 450 :: 360 : 531 | 3+ | 178 : 177 |
| | 27 | 5,03 | 360 | 534 | | | |

Hence

Hence therefore, in comparing different experiments, as some fall short, and others exceed the maximum, and all agree therewith, as near as can be expected, in an affair where so many different circumstances are concerned, we may, according to the laws of reasoning by induction, conclude the maxim true; *viz.* that the effects are nearly as the quantity of water expended.

Maxim II. *That the expence of water being the same, the effect will be nearly as the height of the virtual or effective head.*

This also will appear by comparing the contents of columns 4, 8, and 10, in any of the sets of experiments.

*Example* 1*st*, *of* N°. 2. *and* N°. 24, *viz.*

| N°. | Virt. Head. | Expence. | Effect. |
|---|---|---|---|
| 2 | 15 | 264,7 | 1266 |
| 24 | 4,7 | 262 | 385 |

Now as the expences are not quite equal, we must proportion one of the effects accordingly: thus

by maxim 1st, 262 : 264,7 : : 385 : 389
and by max. 2d, 15 : 4,7 : : 1266 : 397

Difference - - - - - - 8

The effect therefore of N°. 24, compared with N°. 2, is less than according to the present maxim in the ratio of 49 : 50.

The foregoing, and two other similar examples, are comprised in the following Table:

| Examples. | N°. Tab. I. | Virtual Head. | Expence of Water. | Effect. | Comparison. | Variation. | Proportional Variation. |
|---|---|---|---|---|---|---|---|
| 1st | 2 | 15 | 264,7 | 1266 | Max. 1st, 262 : : 264,7 : : 385 : 319<br>Max. 2d, 15 : : 4,7 : : 1266 : 397 | 8 — | 25 : 26 |
| 2d | 1 | 15,85 | 275 | 1411 | Max. 1st, 114 : : 275 : : 117 : 282<br>Max. 2d, 15,85 : : 3,55 : : 1411 : 316 | 34— | 8 : : 9 |
| 3d | 11 | 14,2 | 342 | 1505 | Max. 1st, 167,5 : : 342 : : 212 : 433<br>Max. 2d, 14,2 : : 4,25 : : 1505 : 450 | 17— | 49 : 50 |
| | 17 | 4,25 | 1675 | 212 | | | |

Maxim

**Maxim III.** *That the quantity of Water expended being the same, the effect is nearly as the square of its velocity.*

This will appear by comparing the contents of columns 3, 8, and 10, in any of the sets of experiments; as for

*Example* 1*st*, *of* N°. 2, *with* N°. 24. *viz.*

| N°. | Turns in a min. | Expence. | Effect. |
|---|---|---|---|
| 2 | 86 | 264,7 | 1266 |
| 24 | 48 | 262 | 385 |

The velocity being as the number of turns, we shall have,

by max. 1st, $262 : 264{,}7 :: 385 : 389$

and by max. 3d. $\left\{ \begin{matrix} 86^2 : 48^2 \\ 7396 : 2304 \end{matrix} \right\} : 1266 : 394$

Difference - - - - 5

The effect therefore of N°. 24, compared with N°. 2, is less than by the present maxim in the ratio of 78 : 79.

The foregoing, and three other similar examples, are comprised in the following Table:

| Examples. | N°. Tab. I. | Turns in a minute. | Expence of Water. | Effect. | Comparison. | Variation. | Proportional Variation. |
|---|---|---|---|---|---|---|---|
| 1st | 2 | 86 | 264,7 | 1266 | Max. 1st, 262 : 264,7 :: 385 : 389<br>Max. 3d, $86\frac{1}{2}$ : $48^2$ :: 1266 : 394<br>7396 : 2304 | 5 — | 78 : 79 |
| | 24 | 48 | 262 | 385 | | | |
| 2d | 1 | 88 | 275 | 1411 | Max. 1st, 114 : 275 :: 117 : 282<br>Max. 3d, $88^2$ : $42^2$ :: 1411 : 321<br>7744 : 1764 | 39— | 7 : 8 |
| | 10 | 42 | 114 | 117 | | | |
| 3d | 11 | 84 | 342 | 1505 | Max. 1st, 167,5 : 342 :: 212 : 433<br>Max. 3d, $84^2$ : $46^2$ :: 1505 : 451<br>7056 : 2116 | 18— | 24 : 25 |
| | 17 | 46 | 167,5 | 212 | | | |
| 4th | 18 | 72 | 357 | 1210 | Max. 1st, 228 : 357 :: 317 : 496<br>Max. 3d, $72^2$ : $48^2$ :: 1210 : 538<br>5184 : 2301 | 42— | 12 : 13 |
| | 21 | 48 | 228 | 317 | | | |



Maxim

Maxim IV. *The aperture being the same, the effect will be nearly as the cube of the velocity of the water.*

This also will appear by comparing the contents of columns 3, 8, and 10; as for

*Example* 1st, *of* N°. 1. *and* N°. 10, *viz.*

| N°. | Turns. | Expence. | Effect. |
|---|---|---|---|
| 1 | 88 | 275 | 1411 |
| 10 | 42 | 114 | 117 |

*Lemma.* It must here be observed, that if water passes out of an aperture, in the same section, but with different velocities the expence will be proportional to the velocity; and therefore conversely, if the expense is not proportional to the velocity, the section of the water is not the same.

Now comparing the water discharged with the turns of N°. 1, and 10, we shall have 88 : 42 : : 275 : 131,2; but the water discharged by N°. 10. is only 114 lb. therefore, though the sluice was drawn to the same height in N°. 10, as in N°. 1, yet the section of the water passing out, was less in N°. 10, than N°. 1, in the proportion of 114 to 131,2; consequently had the effective aperture or section of the water been the same in N°. 10, as in N°. 1, so that 131,2 lb. of water had been discharged instead of 114, the effect would have been increased in the same proportion; that is

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| by the *Lemma*, | 88 | : | 42 | : : | 275 | : | 131,2 |
| by maxim 1st, | 114 | : | 131,2 | : : | 117 | : | 134,5 |
| and by max. 4th, | $83^3$ | : | $42^3$ | : : | 1411 | : | 153,5 |
| | 681472 | : | 74088 | | | | |

Difference - - - - 19

The effect therefore of 10, compared with N°. 1, is less than it ought to be by the present maxim in the ratio of 7 : 8.

The foregoing, and three other similar examples, are contained in the following Table.

| Examples. | N°. Tab. I. | Turns in a minute. | Expence of water. | Effect. | Comparison. | Variation. | Proportional Variation. |
|---|---|---|---|---|---|---|---|
| 1st | 1 | 88 | 275 | 1411 | Lemma. 88 : 275 : : 131,2 : 1411 | 19— | 20 : 19 |
| | 10 | 42 | 114 | 117 | Max. 1. 114 : : 134,5 : 117 | | |
| | | | | | Max. 4. $88^3$ : : $153,5^3$ : 1411 | | |
| 2d | 11 | 84 | 342 | 1505 | Lemma. 84 : 342 : : 187,3 : 1505 | 10— | 23 : 24 |
| | 17 | 46 | 167,5 | 212 | Max. 1. $167,5^2$ : : 237 : 212 | | |
| | | | | | Max. 4. $84^3$ : : 247 : $46^3$ | | |
| 3d | 18 | 72 | 357 | 1210 | Lemma. 72 : 357 : : 238 : 1210 | 24— | 14 : 15 |
| | 21 | 48 | 228 | 317 | Max. 1. 228 : : 331 : 317 | | |
| | | | | | Max. 4. $72^3$ : : 355 : $48^3$ | | |
| 4th | 22 | 68 | 359 | 1006 | Lemma. 68 : 359 : : 253,4 : 1006 | 18+ | 7 : 8 |
| | 24 | 48 | 262 | 385 | Max. 1. 262 : : 372 : 385 | | |
| | | | | | Max. 4. $68^3$ : : 354 : $48^3$ | | |

*Observations.*

## *Observations.*

*Observ.* 1*st.* On comparing column 2d and 4th, Tab. I. it is evident that the *virtual head* bears no certain proportion to the *head of water*; but that when the aperture is greater, or the velocity of the water issuing therefrom less, they approach nearer to a coincidence; and consequently in the large openings of mills and sluices, where great quantities of water are discharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

*Observ.* 2*d.* Upon comparing the several proportions between the *power* and *effect* in column 11th, the most general is that of 10 to 3; the extremes 10 to 3,2 and 10 to 2,8; but as it is observable, that where the quantity of water, or the velocity thereof; that is, where the power is greatest, the 2d term of the ratio is greatest also: we may therefore well allow the proportion subsisting in large works, as 3 to 1.

*Observ.* 3*d.* The proportions of *velocities* between the *water* and *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approach to that of 2 to 1, the best general proportion will be that of 5 to 2.

*Observ.* 4*th.* On comparing the numbers in column 13, it appears, that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally stop it; but that they are contained within the limits of 20 to 19, and of 20 to 15; but as the effect approaches nearest to the ratio of 20 to 15, or of 4 to 3, when the power is greatest, whether by increase of velocity, or quantity of water, this seems to be the most applicable to large works; but as the load that a wheel ought to have, in order to work to the best advantage, can be assigned,

assigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it, the exact knowledge of the greatest load it will bear, is of the less consequence in practice.

It is to be noted, that in all the examples under the three last of the four preceding maxims, the effect of the lesser power falls short of its due proportion to the greater, when compared by its maxim; except the last example of maxim 4th: and hence, if the experiments are taken strictly, we must infer, that the effects increase and diminish in an higher ratio than those maxims suppose: but as the deviation is not very considerable, the greatest being about 1-8th of the quantity in question; and as it is not easy to make experiments of so compounded a nature with absolute precision; we may rather suppose, that the lesser power is attended with some friction, or works under some disadvantage, which has not been duly accounted for; and therefore we may conclude, that these maxims will hold very nearly, when applied to works in large.

After the experiments above-mentioned were tried, the wheel, which had originally 24 floats, was reduced to twelve; which caused a diminution in the effect, on account of a greater quantity of water escaping between the floats and the floor; but a circular sweep being adapted thereto, of such a length, that one float entered the curve before the preceding one quitted it, the effect came so near to the former, as not to give hopes of advancing it by increasing the number of floats beyond 24 in this particular wheel.

# PART II.

## *Concerning* OVERSHOT WHEELS.

### *Read before the Royal Society, May* 24, 1759.

IN the former part of this essay, we have considered the impulse of a confined stream, acting on *Undershot Wheels*. We now proceed to examine the power and application of water, when acting by its *gravity* on *Overshot Wheels*.

In reasoning without experiment, one might be led to imagine, that however different the mode of application is; yet that whenever the same quantity of water descends through the same perpendicular space, that the natural effective power would be equal; supposing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the most of it: for if we suppose the height of a column of water to be 30 inches, and resting upon a base or aperture of one inch square, every cubic inch of water that departs therefrom will acquire the same velocity, or *momentum*, from the uniform pressure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; *viz.* such a velocity as, in a contrary direction, would carry it to the level from whence it fell *; one would therefore suppose, that a cubic inch of water, let fall through a space of thirty inches, and there impinging upon another body, would be capable of producing an equal effect by collision, as if the same cubic inch had descended through the same space with a slower motion, and produced its effects gradually: for in both cases gravity acts upon an equal quantity of matter, through an equal

* This is a consequence of the rising of jets to the height of their reservoirs nearly.

space;

space; *and consequently, that whatever was the ratio between the power and effect in undershot wheels, the same would obtain in overshot, and indeed in all others: yet, however conclusive this reasoning may seem, it will appear in the course of the following deductions, that the effect of the gravity of descending bodies is very different from the effect of the stroke of such as are *non elastic*, though generated by an equal mechanical power.

The alterations in the machinery already described, to accommodate the same for experiments on overshot wheels, were principally as follows:

PLATE II. *Fig.* 2. The sluice I *b* being shut down, the rod H I was unscrewed and taken off.

The undershot water-wheel was taken off the axis, and instead thereof on overshot wheel of the same diameter was put into its place. *Note*, This wheel was two inches in the shroud or depth of the bucket, the number of the buckets was 36.

The standards S. and T, *Fig.* 1. were raised half an inch, so that the bottom of the wheel might be clear of stagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, *Fig.* 2. The aperture was adjusted by a shuttle *h i*, which also closed up the outer end of the trunk, when the water was to be stopped.

*Fig.* 3. The ratchet *o o*, not being of one piece of metal with the ferrule *e e*, *i i*, (though so described before, to prevent unnecessary distinctions), was with its catch turned the contrary side; consequently the moveable barrel would do its office equally, notwithstanding the water-wheel, when at work, moved the contrary way.

* Gravity, it is true, acts a longer space of time upon the body that descends slow than upon that which falls quick; but this cannot occasion the difference in the effect: for an elastic body falling through the same space in the same time, will, by collision upon another elastic body, rebound nearly to the height from which it fell; or, by communicating its motion, cause an equal one to ascend to the same height.

*Specimen*

## *Specimen of a Set of Experiments.*

Head 6 inches.

$14\frac{1}{2}$ strokes of the pump in a minute, 12 ditto = 80lb.*
Weight of the scale (being wet) $10\frac{1}{2}$ oz.
Counterweight for 20 turns, besides the scale, 3 oz.

| No. | Weight in the Scale. | Turns. | Product. | Observations. |
|---|---|---|---|---|
| 1 | 0 *lb.* | 60 | | Threw most part of the water out of the wheel. |
| 2 | 1 | 56 | | |
| 3 | 2 | 52 | | |
| 4 | 3 | 49 | 147 | Received the water more quietly. |
| 5 | 4 | 47 | 188 | |
| 6 | 5 | 45 | 225 | |
| 7 | 6 | $42\frac{1}{2}$ | 255 | |
| 8 | 7 | 41 | 287 | |
| 9 | 8 | $38\frac{1}{2}$ | 308 | |
| 10 | 9 | $36\frac{1}{2}$ | $328\frac{1}{2}$ | |
| 11 | 10 | $35\frac{1}{2}$ | 355 | |
| 12 | 11 | $32\frac{3}{4}$ | $360\frac{1}{2}$ | |
| 13 | 12 | $31\frac{1}{4}$ | 375 | |
| 14 | 13 | $28\frac{1}{2}$ | $370\frac{1}{2}$ | |
| 15 | 14 | $27\frac{1}{2}$ | 385 | |
| 16 | 15 | 26 | 390 | |
| 17 | 16 | $24\frac{1}{2}$ | 392 | |
| 18 | 17 | $22\frac{3}{4}$ | $386\frac{3}{4}$ | |
| 19 | 18 | $21\frac{3}{4}$ | $391\frac{1}{2}$ | |
| 20 | 19 | $20\frac{3}{4}$ | $394\frac{1}{4}$ | maximum. |
| 21 | 20 | $19\frac{3}{4}$ | 395 | |
| 22 | 21 | $18\frac{1}{4}$ | $388\frac{1}{4}$ | |
| 23 | 22 | 18 | 396 | Worked irregular. |
| 24 | 23 | Overset by its load. | | |

* The small difference, in the value of 12 strokes of the pump, from the former experiments, was owing to a small difference in the length of the stroke, occasioned by the warping of the wood.

*Reduction*

## *Reduction of the preceding Specimen.*

In these experiments the head being 6 inches, and the height of the wheel 24 inches, the whole descent will be 30 inches: the expence of water was $14\frac{1}{2}$ strokes of the pump in a minute, whereof 12 contained 80 lb.; therefore the water expended in a minute was $96\frac{2}{3}$ lb. which, multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we shall have $20\frac{3}{4}$ turns in a minute, each of which raised the weight $4\frac{1}{2}$ inches, that is, 93,37 inches in a minute. The weight *in* the scale was 19 lb. the weight of the scale $10\frac{1}{2}$ oz.; the counterweight 3 oz. in the scale, which, with the weight of the scale $10\frac{1}{2}$ oz. makes in the whole $20\frac{1}{2}$ lb. which is the whole resistance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The *ratio* therefore of the *power* and *effect* will be as 2900 : 1914, or as 10 : 6,6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we shall have $96\frac{2}{3}$ lb. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 82, or as 5 : 4 nearly.

The reduction of this specimen is set down in N°. 9, of the following Table; and the rest were deducted from a similar set of experiments, reduced in the same manner.

TABLE

**TABLE II.** *containing the Result of Sixteen Sets of Experiments on Overshot Wheels.*

| N°. | Whole descent. | Water expended in a minute. | Turns at the maximum in a min. | Weight raised at the maximum. | Power of the whole descent. | Power of the wheel. | Effect. | Ratio of the whole power and effect. | Ratio of power of the wheel and effect. | Mean ratio. |
|---|---|---|---|---|---|---|---|---|---|---|
| | *Inch* | *lb.* | | *lb.* | | | | | | |
| 1 | 27 | 30 | 19 | $6\frac{1}{2}$ | 810 | 720 | 556 | 10 : 6,9 | 10 : 7,7 | Medium 10:8,1 |
| 2 | 27 | $56\frac{2}{3}$ | $16\frac{1}{4}$ | $14\frac{1}{2}$ | 1530 | 1360 | 1060 | 10 : 6,9 | 10 : 7,8 | |
| 3 | 27 | $56\frac{2}{3}$ | $20\frac{3}{4}$ | $12\frac{1}{2}$ | 1530 | 1360 | 1167 | 10 : 7,6 | 10 : 8,4 | |
| 4 | 27 | $63\frac{1}{3}$ | $20\frac{1}{2}$ | $13\frac{1}{2}$ | 1710 | 1524 | 1245 | 10 : 7,3 | 10 : 8,2 | |
| 5 | 27 | $76\frac{2}{3}$ | $21\frac{1}{2}$ | $15\frac{1}{2}$ | 2070 | 1840 | 1500 | 10 : 7,3 | 10 : 8,2 | |
| 6 | $28\frac{1}{2}$ | $73\frac{1}{3}$ | $18\frac{3}{4}$ | $17\frac{1}{2}$ | 2090 | 1764 | 1476 | 10 : 7, | 10 : 8,4 | 10:8,2 |
| 7 | $28\frac{1}{2}$ | $96\frac{2}{3}$ | $20\frac{1}{4}$ | $20\frac{1}{2}$ | 2755 | 2320 | 1868 | 10 : 6,8 | 10 : 8, | |
| 8 | 30 | 90 | 20 | $19\frac{1}{2}$ | 2700 | 2160 | 1755 | 10 : 6,5 | 10 : 8,1 | 10:8,2 |
| 9 | 30 | $96\frac{2}{3}$ | $20\frac{3}{4}$ | $20\frac{1}{2}$ | 2900 | 2320 | 1914 | 10 : 6,6 | 10 : 8,2 | |
| 10 | 30 | $113\frac{1}{3}$ | 21 | $23\frac{1}{2}$ | 3400 | 2720 | 2221 | 10 : 6,5 | 10 : 8,2 | |
| 11 | 33 | $56\frac{2}{3}$ | $20\frac{1}{4}$ | $13\frac{1}{2}$ | 1870 | 1360 | 1230 | 10 : 6,6 | 10 : 9, | 10:8,5 |
| 12 | 33 | $106\frac{2}{3}$ | $22\frac{1}{4}$ | $21\frac{1}{2}$ | 3520 | 2560 | 2153 | 10 : 6,1 | 10 : 8,4 | |
| 13 | 33 | $146\frac{2}{3}$ | 23 | $27\frac{1}{2}$ | 4840 | 3520 | 2846 | 10 : 5,9 | 10 : 8,1 | |
| 14 | 35 | 65 | $19\frac{3}{4}$ | $16\frac{1}{2}$ | 2275 | 1560 | 1466 | 10 : 6,5 | 10 : 9,4 | 10:8,5 |
| 15 | 35 | 120 | $21\frac{1}{2}$ | $25\frac{1}{2}$ | 4200 | 2880 | 2467 | 10 : 5,9 | 10 : 8,6 | |
| 16 | 35 | $163\frac{1}{2}$ | 25 | $26\frac{1}{2}$ | 5728 | 3924 | 2981 | 10 : 5,2 | 10 : 7,6 | |
| 1. | 2. | 3. | 4. | 5. | 6. | 7. | 8. | 9. | 10. | 11 |

## *Observations and Deductions from the foregoing Experiments.*

### I. *Concerning the Ratio between the Power and Effect of Overshot Wheels.*

The effective power of the water must be reckoned upon the whole descent; because it must be raised that height, in order to be in a condition of producing the same effect a second time.

The

The ratios between the *powers* so estimated, and the *effects* at the *maximum* deduced from the several sets of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that those ratios differ from that of 10 to 7,6 to that of 10 : 5,2, that is, nearly from 4 : 3 to 4 : 2. In those experiments where the heads of water and quantities expended are least, the proportion is nearly as 4 : 3; but where the heads and quantities are greatest, it approaches nearer to that of 4 : 2; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest was 3 : 1; *the effect therefore of overshot wheels, under the same circumstances of quantity and fall, is at a medium double to that of the undershot:* and, as a consequence thereof, *that nonelastic bodies, when acting by their impulse or collision, communicate only a part of their original power;* the other part being spent in changing their figure in consequence of the stroke.

The powers of water computed from the height of the wheel only, compared with the effects, as in column 10, appear to observe a more constant ratio: for if we take the medium of each class, which is set down in column 11, we shall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10 : 8,5; and as the second term of the ratio gradually increases from 8,1 to 8,5, by an increase of head from 3 inches to 11, the excess of 8,5 above 8,1 is to be imputed to the superior impulse of the water at the head of 11 inches above that of 3 inches: so that if we reduce 8,1 to 8, on account of the impulse of the 3 inch head, *we shall have the ratio of the power, computed upon the height of the wheel only, to the effect at a maximum as* 10 : 8, *or as* 5 : 4 *nearly:* and from the quality of the ratio between power and effect, subsisting where the constructions are similar, we must infer, *that the effects*, as well as the powers, *are as the quantities of water and perpendicular heights multiplied together respectively.*

II. *Con-*

## II. *Concerning the most proper Height of the Wheel in proportion to the whole Descent.*

We have already seen, from the preceding observation, that the effect of the same quantity of water, descending through the same perpendicular space, is double, when acting by its gravity upon an overshot wheel, to what the same produces when acting by its impulse upon an undershot. It also appears, that by increasing the head from 3 inches to 11, that is, the whole descent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8,1 to 8,4, that is, as 7 : 7,26; and consequently the increase of effect as not 1-7th of the increase of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole descent, the greater will be the effect;* because it depends less upon the impulse of the head, and more upon the gravity of the water in the buckets: and if we consider how obliquely the water issuing from the head must strike the buckets, we shall not be at a loss to account for the little advantage that arises from the impulse thereof: and shall immediately see of how little consequence this impulse is to the effect of an overshot wheel. However, as every thing has its limits, so has this: for thus much is desirable *that the water shall have somewhat greater velocity, than the circumference of the wheel, in coming thereon;* otherwise the wheel will not only be retarded, by the buckets striking the water, but thereby dashing a part of it over, so much of the power is lost.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requisite to give the water its proper velocity is easily computed from the common rules of hydrostatics; and will be found much less than what is generally practised.

III. *Con-*

## III. *Concerning the Velocity of the Circumference of the Wheel, in order to produce the greatest effect.*

If a body is let fall freely from the surface of the head to the bottom of the descent, it will take a certain time in falling; and in this case the whole action of gravity is spent in giving the body a certain velocity: but if this body in falling is made to act upon some other body, so as to produce a mechanical effect, the falling body will be retarded; because a part of the action of gravity is then spent in producing the effect, and the remainder only giving motion to the falling body: and therefore *the slower a body descends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect*; and in consequence the greater that effect may be.

If a stream of water falls into the bucket of an overshot wheel, it is there retained until the wheel by moving round discharges it: of consequence the slower the wheel moves, the more water each bucket will receive; so that what is lost in speed, is gained by the pressure of a greater quantity of water acting in the buckets at once: and, if considered only in this light, the mechanical power of an overshot wheel to produce effects will be equal whether it moves quick or slow: but if we attend to what has been just now observed of the falling body, it will appear that so much of the action of gravity, as is employed in giving the wheel and water therein a greater velocity, must be subtracted from its pressure upon the buckets, so that, though the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the same in all cases; yet, as each cubic inch, when the velocity is *greater* does not press so much upon the bucket as when it is *less*, the power of the water to produce effects will be greater in the less velocity than in the greater: and hence we are led to this general rule, *that*, cæteris paribus, *the less the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this

this doctrine, together with the limits it is subject to in practice may be deduced from the foregoing specimen of a set of experiments.

From these experiments it appears that, when the wheel made about 20 turns in a minute, the effect was, near upon, the greatest. When it made 30 turns, the effect was diminished about $\frac{1}{20}$ part; but that when it made 40, it was diminished about $\frac{1}{4}$; when it made less than $18\frac{1}{4}$, its motion was irregular; and when it was loaded so as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel should not be diminished further than what will procure some solid advantage in point of power: because, *cæteris paribus*, as the motion is slower, the buckets must be made larger; and the wheel being more loaded with water, the stress upon every part of the work will be increased in proportion: *The best velocity for practice therefore will be such, as when the wheel here used made about 30 turns in a minute; that is, when the velocity of the circumference is a little more than 3 feet in a second.*

Experience confirms, that this velocity of 3 feet in a second is applicable to the highest overshot wheels, as well as the lowest; and all other parts of the work being properly adapted thereto, will produce very nearly the greatest effect possible: however this also is certain from experience, that *high wheels may deviate further from this rule, before they will lose their power, by a given aliquot part of the whole, than low ones can be admitted to do;* for a wheel of 24 feet high may move at the rate of six feet per second without losing any considerable part of its power*;

* The 24 feet wheel going at 6 feet in a second, seems owing to the small proportion that the head (requisite to give the water the proper velocity of the wheel) bears to the whole height.

and on the other hand, I have seen a wheel of 33 feet high, that has moved very steadily and well with a velocity but little exceeding 2 feet.

## IV. *Concerning the Load for an Overshot Wheel, in Order that it may produce a Maximum.*

*The maximum load for an overshot wheel, is that which reduces the circumferences of the wheel to its proper velocity;* and this will be known, by dividing the effect it ought to produce in a given time by the space intended to be described by the circumference of the wheel in the same time: the quotient will be the resistance overcome at the circumference of the wheel; and is equal to the load required, the friction and resistance of the machinery included.

## V. *Concerning the greatest possible Velocity of an Overshot Wheel.*

The greatest velocity that the circumference of an overshot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies; for it is plain that the velocity of the circumference can never be greater, than to describe a semi-circumference, while a body let fall from the top of the wheel will descend through its diameter; nor indeed quite so great, as a body descending through the same perpendicular space cannot perform the same in so small a time when passing through a semi-circle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall through the diameter in one second: this wheel therefore can never arrive at a velocity equal to the making one turn in two seconds; but, in reality, an overshot wheel can never come near this velocity; for when it acquires a certain speed, the greatest part of the water is prevented from entering the buckets;

buckets; and the rest, at a certain point of its descent, is thrown out again by the centrifugal force. This appears to have been the case in the three first experiments of the foregoing specimen; but as the velocity, when this begins to happen, depends upon the form of the buckets, as well as other circumstances, *the utmost velocity of overshot wheels is not to be determined generally:* and, indeed, it is the less necessary in practice, as it is in this circumstance incapable of producing any *mechanical effect*, for reasons already given.

## VI. *Concerning the greatest Load that an Overshot Wheel can overcome.*

*The greatest load an overshot wheel will overcome, considered abstractedly, is unlimited or infinite:* for as the buckets may be of any given capacity, the more the wheel is loaded, the slower it turns; but the slower it turns, the more will the buckets be filled with water; and consequently though the diameter of the wheel and quantity of water expended, are both limited, yet no resistance can be assigned, which it is not able to overcome: but in practice we always meet with something that prevents our getting into infinitesimals; for when we really go to work to build a wheel, the buckets must necessarily be of some given capacity; and consequently *such a resistance will stop the wheel, as is equal to the effort of all the buckets in one semi-circumference filled with water.*

The structure of the buckets being given; the quantity of this effort may be assigned; but is not of much consequence to the practice, as in this case also the wheel loses its power; for though here is the exertion of gravity upon a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no *mechanical effect*, according to our definition. But, in reality, an overshot wheel generally ceases to be useful before it is loaded to that pitch; for *when it meets with*

*such a resistance as to diminish its velocity to a certain degree, its motion becomes irregular; yet this never happens until the velocity of the circumference is less than 2 feet per second, where the resistance is equable*, as appears not only from the preceding specimen, but from experiments on larger wheels.

## SCHOLIUM.

Having now examined the different effects of the power of water, when acting by its *impulse*, and by its *weight*, under the titles of *undershot* and *overshot* wheels; we might naturally proceed to examine the effects when the impulse and weight are combined, as in the several kinds of *breast wheels*, *&c.* but, what has been already delivered being carefully attended to, the application of the same principles in these mixt cases will be easy, and reduce what I have to say on this head into a narrow compass: for all kinds of wheels where the water cannot descend through a given space, unless the wheel moves therewith, are to be considered of the nature of an overshot wheel, according to the perpendicular height that the water descends from; and all those that receive the impulse or shock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be considered as undershots. And therefore a wheel, which the water strikes at a certain point below the surface of the head, and after that descends in the arch of a circle, pressing by its gravity upon the wheel; *the effect of such a wheel will be equal to the effect of an undershot, whose head is equal to the difference of level between the surface of the water in the reservoir and the point where it strikes the wheel, added to that of an overshot, whose height is equal to the difference of level between the point where it strikes the wheel and the level of the tail-water*. It is here supposed, that the wheel receives the shock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmost advantage of both these powers; otherwise a reduction must be made on that account.

Many

Many obvious and considerable improvements upon the common practice naturally offer themselves, from a due consideration of the principles here established, as well as many popular errors shew themselves in view: but as my present purpose extends no farther than the laying down such general rules as will be found to answer in practice, I leave the particular application to the intelligent artist, and to the curious in these matters.

# PART III.

## *On the* CONSTRUCTION *and* EFFECTS *of* WINDMILL SAILS.

*Read before the Royal Society,* 31 *May and* 14 *June* 1759.

IN trying experiments on windmill sails, the wind itself is too uncertain to answer the purpose, we must therefore have recourse to an artificial wind.

This may be done two ways; either by causing the air to move against the machine, or the machine to move against the air. To cause the air to move against the machine, in a sufficient volume, with steadiness and the requisite velocity, is not easily put in practice: To carry the machine forward in a right line against the air, would require a larger room than I could conveniently meet with. What I found most practicable, therefore, was to carry the axis, whereon the sails were to be fixed, progressively round in the circumference of a large circle. Upon this idea * a machine was constructed, as follows:

PLATE

* Some years ago Mr. Rouse, an ingenious gentleman of Harborough, in Leicestershire, set about trying experiments on the velocity of the wind, and force thereof upon plain surfaces and windmill-sails; and, much about the same time, Mr. Ellicott contrived a machine for the use of the late celebrated Mr. B. Robins, for trying the resistance of plain surfaces moving through the air. The machines of both these gentlemen were much alike, though at that time totally unacquainted with each other's inquiries. But it often happens, that when two persons think justly upon the same subject, their experiments are alike. This machine was also built upon the same idea as the foregoing; but differed in having the hand for the first mover, with a pendulum for its regulator, instead of a weight,

as

PLATE III. Fig. 1.

A B C is a pyramidical frame for supporting the moving parts.

D E is an upright axis, whereon is framed

F G, an arm for carrying the sails at a proper distance from the centre of the upright axis.

H is a barrel upon the upright axis, whereon is wound a cord; which, being drawn by the hand, gives a circular motion to the axis, and to the arm F G; and thereby carries the axis of the sails in the circumference of a circle, whose radius is D I, causing thereby the sails to strike the air, and turn round upon their own axis.

At L is fixed the end of a small line, which passing through the pullies M N O, terminates upon a small cylinder or barrel upon the axis of the sails, and, by winding thereon, raises

P the scale, wherein the weights are placed for trying the power of the sails. This scale, moving up and down in the direction of the upright axis, receives no disturbance from the circular motion.

Q R two parallel pillars standing upon the arm F G, for the purpose of supporting and keeping steady the scale P; which is kept from swinging by means of

as in the former; which was certainly best for the purposes of measuring the impulse of the wind, or resistance of plains; but the latter is more applicable to experiments on windmill sails; because every change of position of the same sails will occasion their meeting the air with a different velocity, though urged by the same weight.

S T, two small chains, which hang loosely round the two pillars.

W is a weight for bringing the centre of gravity of the moveable part of the machine into the centre of motion of the axis D E.

V X is a pendulum, composed of two balls of lead, which are moveable upon a wooden rod, and thereby can be so adjusted, as to vibrate in any time required. This pendulum hangs upon a cylindrical wire, whereon it vibrates, as on a rolling axis.

Y is a perforated table for supporting the axis of the pendulum.

*Note*, The pendulum being so adjusted, as to make two vibrations in the time that the arm F G is intended to make one turn; the pendulum being set a vibrating, the experimenter pulls by the cord Z, with sufficient force to make each half revolution of the arm to correspond with each vibration, as equal as possible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it easy to give motion thereto with all the regularity that is necessary.

*Specimen of a Set of Experiments.*

| | |
|---|---|
| Radius of the sails - - - - | 21 inches |
| Length of ditto in the cloth - - - | 18 |
| Breadth of ditto - - - - - | 5,6 |
| * Angle at the extremity - - - | 10 degrees |
| Ditto at the greatest inclination - | 25 |
| | 20 turns |

* In all the following experiments the angle of the sails is accounted from the plain of their motion; that is, when they stand

at

20 turns of the sails raised the weight - 11,3 inches.
Velocity of the centre of the sails, in the circumference of the great circle, in a second - - - - - - } 6 feet 0 inches.
Continuance of the experiment - - 52 seconds.

| No. | Wt. in the scale. | Turns. | Product. | |
|---|---|---|---|---|
| 1 | 0*lb.* | 108 | 0 | |
| 2 | 6 | 85 | 510 | |
| 3 | $6\frac{1}{2}$ | 81 | $526\frac{1}{2}$ | |
| 4 | 7 | 78 | 546 | |
| 5 | $7\frac{1}{2}$ | 73 | $547\frac{1}{2}$ | maximum. |
| 6 | 8 | 65 | 520 | |
| 7 | 9 | 0 | 0 | |

*N. B.* The weight of the scale and pulley was 3 oz.; and that 1 oz. suspended upon one of the radii, at $12\frac{1}{2}$ inches from the centre of the axis, just overcame the friction scale and load of $7\frac{1}{2}$ lb.; and placed at $14\frac{19}{20}$ inches, overcame the same resistances with 9 lb. in the scale.

## *Reduction of the preceding Specimen.*

N°. 5. being taken for the maximum, the weight in the scale was 7 lb. 8 oz. which, with the weight of the scale and pulley 3 oz. makes 7 lb. 11 oz. equal to 123 oz.; this added to the friction of the machinery, the sum is the whole resistance *. The

at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle so denoted, the weather of the sail; which they denominate greater or less, according to the quantity of this angle.

* The resistance of the air is not taken into the account of resistance, because it is inseparable from the application of the power.

friction

friction of the machinery is thus deduced: Since 20 turns of the sails raised the weight 11,3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raised by a single line, the radius of the cylinder being half the former, *viz.* .09, the resistance would have been the same: we shall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the small weight was applied; so is the weight applied to the arm, to a fourth weight, which is equivalent to the sum of the whole resistance together; that is, .09 : 12,5 : : 1 oz. : 139 oz. this exceeds 123 oz. the weight in the scale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8 lb. 11 oz. = 8,69 lb. for the sum of the whole resistance; and this, multiplied by 73 turns, makes a product of 634, which may be called the representative of the *effect* produced.

In like manner, if the weight 9 lb. which caused the sails to rest after being in motion, be augmented by the weight of the scale and its relative friction, it will become 10,37 lb. The result of this specimen is set down in N°. 12. of Table III. and the result of every other set of experiments therein contained were made and reduced in the same manner.

## TABLE III.

# TABLE III.

*Containing Nineteen Setts of Experiments on Windmill-Sails of various Structures, Positions, and Quantities of Surfaces.*

| The kind of sails made use of | N°. | Angle at the extremities. | Greatest angle. | Turns of the sails unloaded. | Turns of ditto at the maximum. | Load at the maximum. | Greatest load. | Product. | Quantity of surface. | Ratio of greatest velocity to the velocity at maxim$^{m}$. | Ratio of greatest load to the load at maximum. | Ratio of surface to the product. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | ° | ° | | | *lb.* | *lb.* | | *Sq In* | | | |
| *Plain* sails at an angle of 55° | 1 | 35 | 35 | 66 | 42 | 7,56 | 12,59 | 318 | 404 | 10:7 | 10:6 | 10:7,9 |
| *Plain* sails weather'd according to the common practice. | 2 | 12 | 12 | | 70 | 6,3 | 7,56 | 441 | 404 | | 10:8,3 | 10:10,1 |
| | 3 | 15 | 15 | 105 | 69 | 6,72 | 8,12 | 464 | 404 | 10:6,6 | 10:8,3 | 10:10,15 |
| | 4 | 18 | 18 | 96 | 66 | 7,0 | 9,81 | 462 | 404 | 10:7, | 10:7,1 | 10:10,15 |
| Weathered according to *Maclaurin's theorem.* | 5 | 9 | $26\frac{1}{2}$ | | 66 | 7,0 | | 462 | 404 | | | 10:11,4 |
| | 6 | 12 | $29\frac{1}{2}$ | | $70\frac{1}{2}$ | 7,35 | | 518 | 404 | | | 10:12,8 |
| | 7 | 15 | $32\frac{1}{2}$ | | $63\frac{1}{2}$ | 8,3 | | 527 | 404 | | | 10:13, |
| Sails weathered in the *Dutch* manner, tried in various positions. | 8 | 0 | 15 | 120 | 93 | 4,75 | 5,31 | 442 | 404 | 10:7,7 | 10:8,9 | 10:11, |
| | 9 | 3 | 18 | 120 | 79 | 7,0 | 8,12 | 553 | 404 | 10:6,6 | 10:8,6 | 10:13,7 |
| | 10 | 5 | 20 | | 78 | 7,5 | 8,12 | 585 | 404 | | 10:9,2 | 10:14,5 |
| | 11 | $7\frac{1}{2}$ | $22\frac{1}{2}$ | 113 | 77 | 8,3 | 9,81 | 639 | 404 | 10:6,8 | 10:8,5 | 10:15,8 |
| | 12 | 10 | 25 | 108 | 73 | 8,69 | 10,37 | 634 | 404 | 10:6,8 | 10:8,4 | 10:15,7 |
| | 13 | 12 | 27 | 100 | 66 | 8,41 | 10,94 | 580 | 404 | 10:6,6 | 10:7,7 | 10:14,4 |
| Sails weathered in the *Dutch* manner, but *enlarged* towards the extremities. | 14 | $7\frac{1}{2}$ | $22\frac{1}{2}$ | 123 | 75 | 10,65 | 12,59 | 799 | 505 | 10:6,1 | 10:8,5 | 10:15,8 |
| | 15 | 10 | 25 | 117 | 74 | 11,08 | 13,69 | 820 | 505 | 10:6,3 | 10:8,1 | 10:16,2 |
| | 16 | 12 | 27 | 114 | 66 | 12,09 | 14,23 | 799 | 505 | 10:5,8 | 10:8,4 | 10:15,8 |
| | 17 | 15 | 30 | 96 | 63 | 12,09 | 14,78 | 762 | 505 | 10:6,6 | 10:8,2 | 10:15,1 |
| 8 sails being *sectors* of *ellipses* in their best positions. | 18 | 12 | 22 | 105 | $64\frac{1}{2}$ | 16,42 | 27,87 | 1059 | 854 | 10:6,1 | 10:5,9 | 10:12,4 |
| | 19 | 12 | 22 | 99 | $64\frac{1}{2}$ | 18,06 | | 1165 | 1146 | 10:5,9 | | 10:10,1 |
| | 1. | 2. | 3. | 4. | 5. | 6. | 7. | 8. | 9. | 10. | 11. | 12. |

To face page 43.

## *Observations and Deductions from the preceding Experiments.*

### I. *Concerning the best Form and Position of Windmill Sails.*

In Table III. N°. 1. is contained the result of a set of experiments upon sails set at the angle which the celebrated Mons. Parint, and succeeding geometricians for many years, held to be the best; *viz.* those whose planes make an angle 55°, nearly with the axis; the complement whereof, or angle that the plane of the sail makes with the plane of their motion, will therefore be 35°, as set down in col. 2. and 3. Now if we multiply their number of turns by the weight they lifted, when working to the greatest advantage, as set down in columns 5. and 6. and compare this product (col. 8.) with the other products contained in the same column, instead of being the greatest, it turns out the least of all the rest. But if we set the angle of the same planes at somewhat less than half the former, or at any angle from 15° to 18°, as in N°. 3. and 4. that is, from 72° to 75° with the axis, the product will be increased in the ratio of 31 : 45; and this is the angle most commonly made use of by practitioners, when the surfaces of the sails are planes.

If nothing more was intended than to determine the most efficacious angle to make a mill acquire motion from a state of rest, or to prevent it from passing into rest from a state of motion, we shall find the position of N°. 1. the best; for if we consult col. 7. which contains the least weights, that would make the sails pass from motion to rest, we shall find that of N°. 1. (relative to the quantity of cloth) the greatest of all. But if the sails are intended, with given dimensions, to produce the greatest effect possible in a given time, we must entirely reject those of N°.

N°. 1. and, *if we are confined to the use of planes, conform ourselves to some angle between* N°. 3. *and* 4, *that is, not less than* 72°, *or greater than* 75°, *with the axis.*

The late celebrated Mr. Maclaurin has judiciously distinguished between the action of the wind upon a sail at rest, and a sail in motion; and, in consequence, as the motion is more rapid near the extremities than towards the centre, that the angle of the different parts of the sail, as they recede from the centre, should be varied. For this purpose he has furnished us with the following theorem *. "Suppose the velocity "of the wind to be represented by $a$, and the velocity of any "given part of the sail to be denoted by $c$; then the effort of "the wind upon that part of the sail will be greatest when the "tangent of the angle, in which the wind strikes it, is to "radius as

"$\sqrt{2 + \frac{9cc}{4aa} + \frac{3c}{2a}}$ to 1." This theorem then assigns the law, by which the angle is to be varied according to the velocity of each part of the sail to the wind: but as it is left undetermined what velocity any one given part of the sail ought to have in respect to the wind, the angle that any one part of the sail ought to have, is left undetermined also; so that we are still at a loss for the proper *data* to apply the theorem. However, being willing to avail myself thereof, and considering that any angle from 15° to 18° was best suited to a plane, and of consequence the best mean angle, I made the sail, at the middle distance between the centre and the extremity, to stand at an angle of 15° 41′ with the plane of the motion; in which case the velocity of that part of the sail, when loaded to a *maximum*, would be equal to that of the wind, or $c = a$. This being determined, the rest were inclined according to the theorem, as follows:

* Maclaurin's account of Sir Isaac Newton's philosophical discoveries, p. 176, art. 29.

Angle

| | | Angle with the axis. | Angle of weather. | |
|---|---|---|---|---|
| Parts of the radius from the centre. | $\frac{1}{6}$ - - $c=\frac{1}{3}a$ | 63° 26′ | 26° 34′ | |
| | $\frac{2}{6}$ - - $c=\frac{2}{3}a$ | 69 54 | 20 6 | |
| | $\frac{1}{2}$ - - $c=a$ | 74 19 | 15 41 | middle. |
| | $\frac{2}{3}$ - - $c=1\frac{1}{3}a$ | 77 20 | 12 40 | |
| | $\frac{5}{6}$ - - $c=1\frac{2}{3}a$ | 79 27 | 10 33 | |
| | 1 - - $c=2a$ | 81 0 | 9 0 | extremity. |

The result hereof was according to N°. 5. being nearly the same as the plane sails, in their best position: but being turned round in their sockets, so that every part of each sail stood at an angle of 3°, and afterwards of 6°, greater than before, that is, their extremities being moved from 9° to 12° and 15°, the products were advanced to 518 and 527 respectively. Now from the small difference between those two products, we may conclude, that they were nearly in their best position, according to N°. 7. or some angle between that and N°. 6. but from these, as well as the plane sails and others, we may also conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the best.*

It is to be observed, that a sail inclined by the preceding rule will expose a convex surface to the wind: whereas the Dutch, and all our modern mill-builders, though they make the angle to diminish, in receding from the centre towards the extremity, yet constantly do it in such a manner, as that the surface of the sail may be concave towards the wind. In this manner the sails made use of in N°. 8, 9, 10, 11, 12, and 13, were constructed; the middle of the sail making an angle with the extreme bar of 12°; and the greatest angle (which was about $\frac{1}{3}$ of the radius from the centre) of 15° therewith. Those sails being tried in various positions, the best appears to be that of N°. 11. where the extremities stood at an angle of $7°\frac{1}{2}$ with the plane of motion, the

product

product being 639 : greater than that of those made by the theorem in the ratio of 9 : 11, and double to that of N°. 1; and this was the greatest product that could be procured without an augmentation of surface. Hence it appears, that *when the wind falls upon a concave surface, it is an advantage to the power of the whole, through every part, taken separately, should not be disposed to the best advantage* *.

Having thus obtained the best position of the sails, or manner of weathering, as it is called by the workmen, the next point was to try what advantage could be made by an addition of surface upon the same radius. For this purpose, the sails made use of had the same weather as those N°. 8. to 13, with an addition to the leading side of each of a triangular cloth, whose height was equal to the height of the sail, and whose base was equal to half the breadth: of consequence the increase of surface upon the whole was one-fourth part, or as 4 : 5. Those sails, by being turned round in their sockets, were tried in four different positions, specified in N°. 14, 15, 16, and 17; from whence it appears, that the best was when every part of the sail made a greater angle by $2°\frac{1}{2}$, with the plane of the motion, than those without the addition, as appears by N°. 15. the product being

* By several trials in large I have found the following angles to answer as well as any. The radius is supposed to be divided into 6 parts and 1-6th, reckoning from the centre, is called 1, the extremity being denoted 6.

| N°. | Angle with the axis. | Angle with the plane of the motion. | |
|---|---|---|---|
| 1 | 72° | 18° | |
| 2 | 71 | 19 | |
| 3 | 72 | 18 | middle. |
| 4 | 74 | 16 | |
| 5 | $77\frac{1}{2}$ | $12\frac{1}{2}$ | |
| 6 | 83 | 7 | extremity |

820:

820: this exceeds 639 more than in the ratio of 4: 5, or that of the increase of cloth. Hence it appears, that *a broader sail requires a greater angle*; and *that when the sail is broader at the extremity, than near the centre, this shape is more advantageous than that of a parallelogram* *.

Many have imagined, that the more sail, the greater the advantage, and have therefore proposed to fill up the whole area: and by making each sail a sector of an ellipsis, according to Monsieur Parint, to intercept the whole cylinder of wind, and thereby to produce the greatest effect possible.

We have therefore proceeded to inquire how far the effect could be increased by a further enlargement of the surface, upon the same radius of which N°. 18 and 19 are specimens. The surfaces indeed were not made planes, and set at an angle of 35°, as Parint proposed; because, from N°. 1, we learn, that this position has nothing to do, when we intend them to work to the greatest advantage. We therefore gave them such an angle as the preceding experiments indicated for such sort of sails, *viz.* 12° at the extremity, and 22° for the greatest weather. By N°. 18. we have the product 1059, greater than N°. 15. in the ratio of 7 : 9; but then the augmentation of cloth is almost 7 : 12. By N°. 19. we have the product 1165, that is greater than N°. 15. as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; consequently had the same quantity of cloth as in

* The figure and proportion of the enlarged sails, which I have found best to answer in large, are represented in the figure, Plate III. where the extreme bar is 1-3d of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular or leading sail is covered with board from the point downwards 1-3d of its height, the rest with cloth as usual. The angles of weather in the preceding note are best for the enlarged sails also; for in practice it is found, that the sails had better have too little than too much weather.

N°. 18, being disposed in a figure similar to that of N°. 15, instead of the product 1059, we should have had the product 1386; and in N°. 19, instead of the product 1165, we should have had a product of 1860; as will be further made appear in the course of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crouded with sail, the less effect is produced in proportion to the surface: and, by pursuing the experiments still further, I found, that though in N°. 19, the surface of all the sails together were not more than 7-8ths of the circular area containing them, yet a further addition rather diminished than increased the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greatest effect for want of proper interstices to escape.*

It is certainly desirable that the sails of windmills should be as short as possible; but at the same time it is equally desirable, that the quantity of cloth should be the least that may be, to avoid damage by sudden squals of wind. The best structure, therefore, for large mills, is that where the quantity of cloth is the greatest, in a given circle, that can be: on this condition, that the effect holds out in proportion to the quantity of cloth; for otherwise the effect can be augmented in a given degree by a lesser increase of cloth upon a larger radius, than would be required, if the cloth was increased upon the same radius. The most useful figure, therefore for practice, is that of N°. 9 or 10, as has been experienced upon several mills in large.

TABLE

**TABLE IV.** *Containing the Result of six Sets of Experiments, made for determining the Difference of Effect, according to the different Velocity of the Wind.*

**N. B. The sails were of the same size and kind as those of N°. 10, 11, and 12, Tab. III. Continuance of the Experiment one minute.**

| N°. | Angle at the extremity. | Velocity of the wind in a second. | Turns of the sails unloaded. | Turns of the sails at maximum. | Load at the maximum. | Greatest load. | Product. | Maximum load for the half velocity. | Turns of the sails therewith. | Product of lesser load and greater velocity. | Ratio of the two products. | Ratio of the greatest load to the load at a maximum. | Ratio of the greatest velocity to the velocity at a maxim$^{m}$. | Ratio of the greatest load at a maximum. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | f. in. | | | *lb.* | *lb.* | | | | | | | | |
| 1 | 5° | 4 4½ | 96 | 66 | 4,47 | 5,37 | 295 | — | — | — | — — | 10 : 6,9 | 10 : 8,3 | |
| 2 | 5 | 8 9 | 207 | 122 | 16,42 | 18,06 | 2003 | 4,47 | 180 | 805 | 10 : 27,3 | 10 : 5,9 | 10 : 9,1 | |
| 3 | 7½ | 4 4½ | — | 65 | 4,62 | — | 300 | — | — | — | — — | —— | —— | |
| 4 | 7½ | 8 9 | — | 130 | 17,52 | — | 2278 | 4,62 | 180 | 832 | 10 : 27,8 | —— | —— | |
| 5 | 10 | 4 4½ | 91 | 61 | 5,03 | 5,87 | 307 | — | — | — | — — | 10 : 6,7 | 10 : 8,5 | |
| 6 | 10 | 8 9 | 178 | 110 | 18,61 | 21,34 | 2047 | 5,03 | 158 | 795 | 10 : 26 | 10 : 6,2 | 10 : 8,7 | |
| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | |

II. *Concerning the ratio between the velocity of windmill sails unloaded, and their velocity when loaded to a maximum.*

Those ratios, as they turned out in experiments upon different kinds of sails, and with different inclinations (the velocity of the wind being the same) are contained in column 10 of tab. III. where the extremes differ from the ratio of 10 : 7,7 to that of 10 : 5,8; but *the most general ratio of the whole will be nearly as* 3 : 2. This ratio also agrees sufficiently near with experiments where the velocity of the wind was different, as in those contained in tab. IV. col. 13. in which the ratios differ from 10 : 6,9 to that of 10 : 5,9. However, it appears in general, that where the power is greater, whether by an enlargement of surface, or a greater velocity of the wind, that the second term of the ratio is less.

III. *Concerning the ratio between the greatest load that the sails will bear without stopping, or what is nearly the same thing, between the least load that will stop the sails, and the load at the maximum.*

Those ratios for different kinds of sails and inclinations, are collected in col. 11. tab. III. where the extremes differ from the ratio of 10 : 6 to that of 10 : 9,2; but taking in those sets of experiments only, where the sails respectively answered best, *the ratios will be confined between that of* 10 : 8 *and of* 10 : 9; *and at a medium about* 10 : 8,3 *or* 6 : 5. This ratio also agrees nearly with those in col. 14. of tab. IV. However it appears, upon the whole, that in those instances, where the angle of the sails or quantity of cloth were greatest, that the second term of the ratio was less.

IV. *Con-*

## IV. *Concerning the effects of sails, according to the different velocity of the wind.*

**Maxim 1.** *The velocity of windmill sails, whether unloaded, or loaded, so as to produce a maximum, is nearly as the velocity of the wind, their shape and position being the same.*

This appears by comparing together the respective numbers of columns 4 and 5, tab. IV. wherein those of numbers 2, 4, and 6, ought to be double of numbers 1, 3, and 5: but as the deviation is no where greater than what may be imputed to the inaccuracy of the experiments themselves, and hold good exactly in numbers 3 and 4; which sets were deduced from the medium of a number of experiments, carefully repeated the same day, and on that account are most to be depended upon, we may therefore conclude the maxim true.

Maxim 2. *The load at the maximum is nearly, but somewhat less than, as the square of the velocity of the wind, the shape and position of the sails being the same.*

This appears by comparing together the numbers in col. 6. tab. IV. wherein those of numbers 2, 4, and 6 (as the velocity is double), ought to be quadruple of those of numbers, 1, 3, and 5; instead of which they fall short, number 2 by $\frac{1}{14}$, number 4 by $\frac{1}{19}$, and number 6 by $\frac{1}{13}$ part of the whole. The greatest of those deviations is not more considerable than might be imputed to the unavoidable errors in making the experiments: but as those experiments, as well as those of the greatest load, all deviate the same way; and also coincide with some experiments communicated to me by Mr. Rouse upon the resistance of planes; I am led to suppose a small deviation, whereby the load falls short of the squares of the velocity; and since the experiments N°. 3 and 4, are most to be depended upon, we must conclude,

that when the velocity is double, the load falls short of its due proportion by $\frac{1}{19}$, or, for the sake of a round number, by about $\frac{1}{20}$ part of the whole.

Maxim 3d. *The effects of the same sail at a maximum are nearly, but somewhat less than, as the cubes of the velocity of the wind.*

It has already been proved, Maxim 1st, that the velocity of sails at the *maximum*, is nearly as the velocity of the wind; and by Maxim 2d, that the load at the *maximum* is nearly as the square of the same velocity: if those two maximums would hold precisely, it would be a consequence that the effect would be in a triplicate ratio thereof: how this agrees with experiment will appear by comparing together the products in col. 8 of tab. IV. wherein those of N°. 2, 4, and 6, (the velocity of the wind being double) ought to be octuple of those of N°. 1, 3, and 5, instead of which they fall short, N°. 2, by $\frac{1}{7}$ N°. 4 by $\frac{1}{20}$, and No. 6, by $\frac{1}{6}$ part of the whole. Now, if we rely on N°. 3, and 4, as the turns of the sails are as the velocity of the wind; and since the load of the maximum falls short of the square of the velocity by about $\frac{1}{20}$ part of the whole: the product made by the multiplication of the turns into the load, must also fall short of the triplicate ratio by about $\frac{1}{20}$ part of the whole product.

Maxim 4th. *The load of the same sails at the maximum is nearly as the squares, and their effect as the cubes of their number of turns in a given time.*

This maxim may be esteemed a consequence of the three preceding; for if the turns of the sails are as the velocity of the wind, whatever quantities are in any given ratio of the velocity of the wind, will be in the same given ratio of the turns of the sails: and therefore, if the load at the *maximum* is as the square, or the effect as the cube, of the velocity of the wind, wanting $\frac{1}{20}$ part

part when the velocity is double; the load at the *maximum* will also be as the square, and the effect as the cube, of the number of turns of the sails in a given time, wanting in like manner $\frac{1}{20}$ part when the number of turns are double in the same time. In the present case, if we compare the loads at the *maximum*, col. 6. with the squares of the number of turns, col 5. of N°. 1 and 2, 5 and 6, or the products of the same numbers col. 8, with the cubes of the number of turns, col. 5, instead of falling short, as N°. 3 and 4, they exceed those ratios: but as the sets of experiments N°. 1 and 2, of 5 and 6, are not to be esteemed of equal authority with those of N°. 3 and 4, we must not rely upon them further than to observe, that *in comparing the gross effects of large machines, the direct proportion of the squares and cubes respectively, will hold as near as the effects themselves can be observed;* and therefore be sufficient for practical estimation, without any allowance.

Maxim 5th. *When sails are loaded so as to produce a maximum at a given velocity, and the velocity of the wind increases, the load continuing the same;* 1*stly*, *The increase of effect, when the increase of the velocity of the wind is small, will be nearly as the squares of those velocities:* 2*dly*, *When the velocity of the wind is double, the effects will be nearly as* 10 : $27\frac{1}{2}$: *But,* 3*dly*, *When the velocities compared, are more than double of that where the given load produces a maximum, the effects increase nearly in a simple ratio of the velocity of the wind.*

It has already been proved, maxim 1st and 2d, that when the velocity of the wind is increased, the turns of the sails will increase in the same proportion, even when opposed by a load as the square of the velocity: and therefore if wanting the opposition of an increase of load, as the square of the velocity, the turns of the sails will again be increased in a simple ratio of the velocity of the wind on that account also; that is, the load continuing the same, the turns of the sails in a given time will be as

the square of the velocity of the wind; and the effect, being in this case as the turns of the sails will be as the square of the velocity of the wind also; but this must be understood only of the first increments of the velocity of the wind: for,

2dly, As the sails will never acquire above a given velocity in relation to the wind, though the load was diminished to nothing; when the load continues the same, the more the velocity of the wind increases (though the effect will continue to increase) yet the more it will fall short of the square of the velocity of the wind; so that when the velocity of the wind is double, the increase of effect, instead of being as 1 : 4, according to the squares, it turns out as 10 : $27\frac{1}{2}$, as thus appears. In tab. IV. col. 9. the loads of N°. 2, 4, and 6, are the same as the maximum loads in col. 6. of N°. 1, 3, and 5. The number of turns of the sails with those loads, when the velocity of the wind is double, are set down in col. 10. and the products of their multiplication in col. 11: those being compared with the products of N°. 1, 3, and 5, col. 8. furnish the ratios set down in col. 12, which at a medium (due regard being had to N°. 3 and 4.) will be nearly as 10 : $27\frac{1}{2}$. 3dly, The load continuing the same, grows more and more inconsiderable, respecting the power of the wind as it increases in velocity; so that the turns of the sails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the simple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the sails, when loaded to a maximum, will be double also; but, *unloaded*, will be no more than triple, by deduction 2d: and therefore the product could not have increased beyond the ratio of 10 : 30 (instead of 10 : $27\frac{1}{2}$) even supposing the sails not to have been retarded at all by carrying the maximum load for the half velocity. Hence we see, that when the velocity of the wind exceeds the double of that, where a constant load produces a maximum, that the increase of effect, which follows the increase of the velocity of the sails, will be nearly as the velocity of the wind, and ultimately in that ratio precisely,

precisely. Hence also we see that windmills, such as the different species for raising water for drainage, &c. lose much of their full effect, when acting against one invariable opposition.

## V. *Concerning the effects of sails of different magnitudes the structure and position being similar, and the velocity of the wind the same.*

Maxim 6. *In sails of a similar figure and position, the number of turns in a given time will be reciprocally as the radius or length of the sail.*

The extreme bar having the same inclination to the plain of its motion, and to the wind; its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and therefore, whatever be the radius, the absolute velocity of the extremity of the sail will be the same: and this will hold good respecting any other bar, whose inclination is the same, at a proportionable distance from the centre; it therefore follows, that the extremity of all similar sails, with the same wind, will have the same absolute velocity; and therefore take a space of time to perform one revolution in proportion to the radius; or, which is the same thing, the number of revolutions in the same given time, will be reciprocally as the length of the sail.

Maxim 7. *The load at a maximum that sails of a similar figure and position will overcome, at a given distance from the center of motion, will be as the cube of the radius.*

Geometry informs us, that in similar figures the surfaces are as the squares of their similar sides; of consequence the quantity of cloth will be as the square of the radius: also in similar figures and positions, the impulse of the wind, upon every similar section of the cloth, will be in proportion to the

surface

surface of that section; and, consequently, the impulse of the wind upon the whole, will be as the surface of the whole: but as the distance of every similar section, from the center of motion, will be as the radius; the distance of the center of power of the whole, from the center of motion, will be as the radius also; that is, the lever by which the power acts, will be as the radius: as therefore the impulse of the wind, respecting the quantity of cloth, is as the square of the radius, and the lever, by which it acts, as the radius simply; it follows, that the load which the sails will overcome, at a given distance from the center, will be as the cube of the radius.

Maxim 8. *The effect of sails of similar figure and position, are as the square of the radius.*

By maxim 6. it is proved, that the number of revolutions made in a given time, are as the radius inversely. Under maxim 7. it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore these equal and opposite ratios destroy one another: but as in similar figures the quantity of cloth is as the square of the radius, and the action of the wind is in proportion to the quantity of cloth, as also appears under maxim 7; it follows that the effect is as the square of the radius.

COROL. 1. Hence it follows, that augmenting the length of the sail, without augmenting the quantity of cloth, does not increase the power; but because what is gained by the length of the lever, is lost by the slowness of the rotation.

COROL. 2. If sails are increased in length, the breadth remaining the same, the effect will be as the radius.

## VI. *Concerning the Velocity of the Extremities of Wind-mill Sails, in Respect to the Velocity of the Wind.*

Maxim 9. *The velocity of the extremities of* Dutch *sails, as well as of the enlarged sails, in all their usual positions when unloaded, or even loaded to a maximum, are considerably quicker than the velocity of the wind.*

The *Dutch* sails unloaded, as in tab. III. N°. 8, made 120 revolutions in 52″: the diameter of the sails being 3 feet 6 inches, the velocity of their extremities will be 25,4 feet in a second; but the velocity of the wind producing it, being 6 feet in the same time, we shall have 6 : 25,4 : : 1 : 4,2; in this case therefore, the velocity of their extremities was 4,2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the same sails, when loaded to a *maximum*, making then 93 turns in 52″, will be found to be as 1 : 3,3 ; or 3,3 times quicker than that of the wind.

The following tables contains 6 examples of *Dutch* sails, and four examples of the enlarged sails, in different positions, but with the constant velocity of the wind of 6 feet in a second, from table 3 : and also 6 examples of *Dutch* sails in different positions, with different velocities of the wind from table IV.

TABLE

TABLE V. *Containing the Ratio of the Velocity of the Extremities of Windmill Sails to the Velocity of the Wind.*

| N° | N° of Tab. III. and IV. | Angle at the Extremity. | Velocity of the wind in a second. | Ratio of the velocity of the wind and extremities of the sails. | | |
|---|---|---|---|---|---|---|
| | | | | unloaded. | loaded. | |
| 1 | 8 | 0° | 6 0 | 1 : 4,2 | 1 : 3,3 | From Table III. |
| 2 | 9 | 3 | 6 0 | 1 : 4,2 | 1 : 2,8 | |
| 3 | 10 | 5 | 6 0 | — — | 1 : 2,75 | |
| 4 | 11 | 7½ | 6 0 | 1 : 4, | 1 : 2,7 | |
| 5 | 12 | 10 | 6 0 | 1 : 3,8 | 1 : 2,6 | |
| 6 | 13 | 12 | 6 0 | 1 : 3,5 | 1 : 2,3 | |
| 7 | 14 | 7½ | 6 0 | 1 : 4,3 | 1 : 2,6 | |
| 8 | 15 | 10 | 6 0 | 1 : 4,1 | 1 : 2,6 | |
| 9 | 16 | 12 | 6 0 | 1 : 4, | 1 : 2,3 | |
| 10 | 17 | 15 | 6 0 | 1 : 3,35 | 1 : 2,2 | |
| 11 | 1 | 5 | 4 4½ | 1 : 4, | 1 : 2,8 | From Tab. IV. |
| 12 | 2 | 5 | 8 9 | 1 : 4,3 | 1 : 2,6 | |
| 13 | 3 | 7½ | 4 4½ | — — | 1 : 2,8 | |
| 14 | 4 | 7½ | 8 9 | — — | 1 : 2,7 | |
| 15 | 5 | 10 | 4 4½ | 1 : 3,8 | 1 : 2,6 | |
| 16 | 6 | 10 | 8 9 | 1 : 3,4 | 1 : 2,3 | |
| 1 | 2 | 3 | 4 | 5 | 6 | |

It appears from the preceding collection of examples, that when the extremities of the *Dutch* sails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in *England*, that their velocity, unloaded, is above 4 times, and loaded to a *maximum*, above 3 times greater than that of the wind: but that when the Dutch sails, or enlarged sails, are

in

in their best positions, their velocity unloaded is 4 times, and loaded to a *maximum*, at a medium the Dutch sails are 2,7, and the enlarged sails 2,6 times greater than the velocity of the wind. Hence we are furnished with a method of knowing the velocity of the wind, from observing the velocity of the windmill sails; for knowing the radius, and the number of turns in a minute, we shall have the velocity of the extremities; which, divided by the following divisors, will give the velocity of the wind.

| | | |
|---|---|---|
| Dutch sails in their common position | unloaded | 4.2 |
| | loaded — | 3.3 |
| Dutch sails in their best position | unloaded | 4.0 |
| | loaded — | 2.7 |
| Enlarged sails in their best position | unloaded | 4.0 |
| | loaded — | 2.6 |

From the above divisors there arises the following compendiums; supposing the radius to be 30 feet, which is the most usual length in this country, and the mill to be loaded to a *maximum*, as is usually the case with corn mills; for every 3 *turns in a minute, of the Dutch sails in their common position, the wind will move at the rate of 2 miles an hour;* for every 5 *turns in a minute, of the Dutch sails in their best position, the wind moves four miles an hour;* and for every 6 *turns in a minute, of the enlarged sails in their best position, the wind will move 5 miles an hour.*

The following table, which was communicated to me by my friend Mr. Rouse, and which appears to have been constructed with great care, from a considerable number of facts and experiments, and which having relation to the subject of this article; I here insert it as he sent it to me: but at the same time must observe, that the evidence for those numbers where the velocity of the wind exceeds 50 miles an hour, do not seem of equal authority with those of 50 miles an hour and under. It is also to

be

be observed, that the numbers in col. 3. are calculated according to the square of the velocity of the wind, which, in moderate velocities, from what has been before observed, will hold very nearly.

**TABLE VI.** *containing the Velocity and Force of Wind, according to their common Appellations.*

| Velocity of the wind. | | Perpendicular force on one foot area in pounds averdupois. | Common appellations of the force of winds. |
|---|---|---|---|
| Miles in one Hour. | Feet in one second. | | |
| 1 | 1,47 | ,005 | Hardly perceptible. |
| 2 | 2,93 | ,020 | Just perceptible. |
| 3 | 4,40 | ,044 | |
| 4 | 5,87 | ,079 | Gentle pleasant wind. |
| 5 | 7,33 | ,123 | |
| 10 | 14,67 | ,492 | Pleasant brisk gale. |
| 15 | 22,00 | 1,107 | |
| 20 | 29,34 | 1,968 | Very brisk. |
| 25 | 36,67 | 3,075 | |
| 30 | 44,01 | 4,429 | High winds. |
| 35 | 51,34 | 6,027 | |
| 40 | 58,68 | 7,873 | Very high. |
| 45 | 66,01 | 9,963 | |
| 50 | 73,35 | 12,300 | A storm or tempest. |
| 60 | 88,02 | 17,715 | A great storm. |
| 80 | 117,36 | 31,490 | An hurricane. |
| 100 | 146,70 | 49,200 | An hurricane that tears up trees, carries buildings before it, &c. |
| 1 | 2 | 3 | |

## VII. *Concerning the absolute Effect, produced by a given Velocity of the Wind, upon Sails of a given Magnitude and Construction.*

It has been observed by practitioners, that in mills with Dutch sails in the common position, that when they make about 13 turns in a minute, they then work at a mean rate: that is, by the compendiums in the last article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a second; which, in common phrase, would be called a *fresh gale*.

The experiments sat down in Tab. IV. N°. 4. were tried with a wind, whose velocity was $8\frac{3}{4}$ feet in a second; consequently had those experiments been tried with a wind, whose velocity was $12\frac{2}{3}$ feet in a second, the effect, by maxim 3d, would have been 3 times greater; because the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{3}{4}$.

From Tab. IV. N°. 4. we find that the sails, when the velocity of the wind was $8\frac{3}{4}$ feet in a second, made 130 revolutions in a minute, with a load of 17,52 lb. From the measures of the machine, preceding the specimen of a set of experiments, we find, that 20 revolutions of the sails raised the scale and weight 11,3 inches: 130 revolutions will therefore raise the scale 73,45 inches, which, multiplied by 17,52 lb. makes a product of 1287, for the effect of the Dutch sails in their best position; that is, when the velocity of the wind is $8\frac{3}{4}$ feet in a second: this product therefore multiplied by three, will give 3861 for the effect of the same sails, when the velocity of the wind is $12\frac{2}{3}$ feet in a second.

Desaguliers makes the utmost power of a man, when working so as to be able to hold it for some hours, to be equal to that

of

of raising an hogshead of water 10 feet high in a minute. Now, an hogshead consisting of 63 ale gallons, being reduced into pounds avoirdupois, and the height into inches; the product made by multiplying those two numbers will be 76800; which is 19 times greater than the product of the sails last-mentioned, at $12\frac{1}{2}$ feet in a second: therefore, by maxim 8th, if we multiply the square root of 19, that is 4,46, by 21 inches, the length of the sail producing the effect 3861, we shall have 93,66 inches, or 7 feet $9\frac{2}{3}$ inches for the radius of a Dutch sail in its best position, whose mean power shall be equal to that of a man: but if they are in their common position, their length must be increased in the ratio of the square root of 442 to that of 639, as thus appears;

The ratio of the *maximum* products of N°. 8 and 11. Tab. III. are as 442 : 639: but by maxim 8, the effects of sails of different radii are as the square of the radii; consequently the square roots of the products or effects, are as the radii simply; and therefore as the square root of 442 is to that of 639; so is 93,66 to 112,66; or 9 feet $4\frac{2}{3}$ inches.

If the sails are of the enlarged kind, then from Tab. III. N°. 11. and 15. we shall have the square root of 820 to that of 639 : : 93,66 : 82,8 inches, or 6 feet $10\frac{3}{4}$ inches: so that in round numbers we shall have the radius of a sail, of a similar figure to their respective models, whose mean power shall be equal to that of a man;

| | |
|---|---|
| The Dutch sails in their common position | $9\frac{1}{2}$ feet. |
| The Dutch sails in their best position - | 8 |
| The enlarged sails in their best position | 7 |

Suppose now the radius of a sail to be 30 feet, and to be constructed upon the model of the enlarged sails, N°. 14 or 15. Tab. III. dividing 30 by 7, we shall have 4,28, the square of which is

is 18,3; and this, according to maxim 7, will be the relative power of a sail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet sail will be equal to the power of 18,3 men, or of $3\frac{2}{3}$ horses; reckoning 5 men to a horse: whereas the effect of the common Dutch sails, of the same length, being less in the proportion of 820 : 442, will be scarce equal to the power of 10 men, or of 2 horses.

That these computations are not merely speculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged sails of 30 feet, applied to the crushing of rape seed, by means of two runners upon the edge, for making oil; I observed, that when the sails made 11 turns in a minute, in which case the velocity of the wind was about 13 feet in a second, according to article 6th, that the runners then made 7 turns in a minute: whereas 2 horses, applied to the same two runners, scarcely worked them at the rate of $3\frac{1}{2}$ turns in the same time. Lastly, with regard to the real superiority of the enlarged sails, above the Dutch sails as commonly made, it has sufficiently appeared, not only in those cases where they have been applied to new mills, but where they have been substituted in the place of the others.

## VIII. *Concerning horizontal Windmills and Water-Wheels, with oblique Vanes.*

Observations upon the effects of common windmills, with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulse, like a ship sailing before the wind, it would be a very great improvement in point of power: while others attending to the extraordinary and even unexpected effects of oblique vanes have been led to imagine that oblique vanes applied to water-mills, would as much exceed the common water wheels, as the vertical wind-mills are found to have exceeded all attempts towards an horizontal

zontal one. Both these notions, but especially the first, have so plausible an appearance, that of late years there has seldom been wanting those, who have assiduously employed themselves to bring to bear designs of this kind: it may not therefore be unacceptable to endeavour to set this matter in a clear light.

PLATE III. fig. 2. Let A B be the section of a plane, upon which let the wind blow in the direction C D, with such a velocity as to describe a given space B E, in a given time (suppose 1 second); and let A B be moved parallel to itself, in the direction C D. Now, if the plane A B moves with the same velocity as the wind; that is, if the point B moves through the space B E in the same time that a particle of air would move through the same space; it is plain that, in this case, there can be no pressure or impulse of the wind upon the plane: but if the plane moves slower than the wind, in the same direction, so that the point B may move to F, while a particle of air, setting out from B at the same instant, would move to E, then B F will express the velocity of the plane; and the relative velocity of the wind and plane will be expressed by the line F. E. Let the ratio of F E to B E be given (suppose 2 : 3); let the line A B represent the impulse of the wind upon the plane A B, when acting with its whole velocity B E; but, when acting with its relative velocity F E, let its impulse be denoted by some aliquot part of A B, as for instance $\frac{4}{9}$ A B: then will $\frac{4}{9}$ of the parallelogram A F represent the mechanical power of the plane; that is, $\frac{4}{9}$ A B $\times$ $\frac{1}{3}$ B E.

2dly, Let I N be the section of a plane, inclined in such a manner, that the base I K of the rectangle triangle I K N may be equal to A B; and the perpendicular N K = B E; let the plane I N be struck by the wind, in the direction L M, perpendicular to I K: then, according to the known rules of oblique forces, the impulse of the wind upon the plain I N, tending to move it according to the direction L M, or N K, will be

be denoted by the base I K; and that part of the impulse, tending to move it according to the direction I K, will be expressed by the perpendicular N K. Let the plane I N be moveable in the direction of I K only; that is, the point I in the direction of I K, and the point N in the direction N Q, parallel thereto. Now it is evident, that if the point I moves through the line I K, while a particle of air, setting forwards at the same time from the point N, move through the line N K, they will both arrive at the point K at the same time; and, consequently, in this case also, there can be no pressure or impulse of the particle of the air upon the plane I N. Now let I O be to I K as B F to B E; and let the plane I N move at such a rate, that the point I may arrive at O, and acquire the position I Q, in the same time that a particle of wind would move through the space N K: as O Q is parallel to I N; (by the properties of similar triangles) it will cut N K in the point P, in such a manner, that N P = B F, and P K = F E: hence it appears, that the plane I N, by acquiring the position O Q, withdraws itself from the action of the wind, by the same space N P, that the plane A B does by acquiring the position F G; and consequently, from the equality of P K to F E, the relative impulse of the wind P K, upon the plane O Q, will be equal to the relative impulse of the wind F E upon the plane F G: and since the impulse of the wind upon A B, with the relative velocity F E, in the direction B E, is represented by $\frac{4}{9}$ A B; the relative impulse of the wind upon the plane I N, in the direction N K, will in like manner be represented by $\frac{4}{9}$ I K; and the impulse of the wind upon the plane I N, with the relative velocity P K, in the direction I K, will be represented by $\frac{4}{9}$ N K: and consequently the mechanical power of the plane I N, in the direction I K, will be $\frac{4}{9}$ the parallelogram I Q: that is $\frac{1}{3}$ I K $\times$ $\frac{4}{9}$ N K: that is, from the equality of I K = A B and N K = B E, we shall have $\frac{4}{9}$ I Q = $\frac{1}{3}$ A B $\times$ $\frac{4}{9}$ B E = $\frac{4}{9}$ A B $\times$ $\frac{1}{3}$ B E = $\frac{4}{9}$ of the area of the parallelogram A F. Hence we deduce this

## General Proposition.

*That all planes, however situated, that intercept the same section of the wind, and having the same relative velocity, in regard to the wind, when reduced into the same direction, have equal powers to produce mechanical effects.*

For what is lost by the obliquity of the impulse, is gained by the velocity of the motion.

Hence it appears, that an oblique sail is under no disadvantage in respect of power, compared with a direct one; except what arises from a diminution of his breadth, in respect to the section of the wind: the breadth I N being by obliquity reduced to I K.

The disadvantage of horizontal windmills therefore does not consist in this; that each sail, when directly exposed to the wind is capable of a less power, than an oblique one of the same dimensions; but that in an horizontal windmill, little more than one sail can be acting at once: whereas in the common windmill, all the four act together: and therefore, supposing each vane of an horizontal windmill, of the same dimensions as each vane of the vertical, it is manifest the power of a vertical mill with four sails, will be four times greater than the power of the horizontal one, let its number of vanes be what it will: this disadvantage arises from the nature of the thing; but if we consider the further disadvantage, that arises from the difficulty of getting the sails back again against the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common sort; as has appeared in some attempts of this kind.

In

In like manner, as little improvement is to be expected from water-mills with oblique vanes: for the power of the same section of a stream of water, is not greater when acting upon an oblique vane, than when acting upon a direct one: and any advantage that can be made by intercepting a greater section, which sometimes may be done in the case of an open river, will be counterbalanced by the superior resistance, that such vanes would meet with by moving at right angles to the current: whereas the common floats always move with the water nearly in the same direction.

Here it may reasonably be asked, that since our geometrical demonstration is general, and proves, that one angle of obliquity is as good as another, why in our experiments it appears, that there is a certain angle which is to be preferred to all the rest? It is to be observed, that if the breadth of the sail I N is given, the greater the angle K I N, and the less will be the base I K: that is, the section of wind intersected, will be less: on the other hand, the more acute the angle K I N, the less will be the perpendicular K N: that is, the impulse of the wind, in the direction I K being less, and the velocity of the sail greater; the resistance of the medium will be greater also. Hence therefore, as there is a diminution of the section of the wind intercepted on one hand, and an increase of resistance on the other, there is some angle, where the disadvantage arising from these causes upon the whole is the least of all; but as the disadvantage arising from resistance is more of a physical than geometrical consideration, the true angle will best be assigned by experiment.

## SCHOLIUM.

In trying the experiments contained in Tab. III. and IV. the different specific gravity of the air, which is undoubtedly different at different times, will cause a difference in the load, proportional to the difference of its specific gravity, though its

velocity remains the same; and a variation of specific gravity may arise not only from a variation of the weight of the whole column, but also by the difference of heat of the air concerned in the experiment, and possibly of other causes; yet the irregularities that might arise from a difference of specific gravity were thought to be too small to be perceivable, till after the principal experiments were made, and their effects compared; from which, as well as succeeding experiments, those variations were found to be capable of producing a sensible, though no very considerable effect: however, as all the experiments were tried in the summer season, in the day-time, and under cover we may suppose that the principal source of error would arise from the different weight of the column of the atmosphere at different times; but as this seldom varies above $\frac{1}{115}$ part of the whole, we may conclude, that though many of the irregularities contained in the experiments referred to in the foregoing essay, might arise from this cause; yet as all the principal conclusions are drawn from the medium of a considerable number, many whereof were made at different times, it is presumed that they will nearly agree with the truth, and be altogether sufficient for regulating the practical construction of those kind of machines, for which use they were principally intended.

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AN

# EXPERIMENTAL EXAMINATION

OF THE QUANTITY AND PROPORTION OF

# MECHANIC POWER

NECESSARY TO BE EMPLOYED IN GIVING DIFFERENT DEGREES OF VELOCITY TO

# HEAVY BODIES

FROM A

## STATE OF REST.

By Mr. JOHN SMEATON, F. R. S.

# HEVAX BODIES

# MECHANIC BOMES

# EXPERIMENTAL EXAMINATION, &c.

*Read before the Royal Society, April 25, 1776.*

ABOUT the year 1686 Sir ISAAC NEWTON first published his *Principia*, and, conformably to the language of mathematicians of those times defined, that "the quantity of "motion is the measure of the same, arising from the velocity "and quantity of matter conjointly." Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. *per* second; and if gravity is suffered to act

upon it for two seconds, it will have, in that time, acquired a velocity that would carry it, at an uniform rate, just double of the former; that is, at the rate of 64 ft. 4 in. *per* second. Now, if in consequence of this equal increase of velocity, in an equal increase of time, by the continuance of the same impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the same impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition above mentioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cause, the impression it will make upon an uniformly resisting medium, or upon uniformly yielding substances, will be as the mass of matter of the moving body, multiplied by the square of its velocity: the question, therefore, properly is, whether those terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been esteemed synonimous, are with the most propriety of language to be esteemed equal, double, or triple, when they have been generated by an equable impulse, acting for an equal double, or triple time; or that it should be measured by the effects being equal, double, or triple, in overcoming resistances before a body in motion can be stopped? For, according as those terms are understood in this or that way, it will necessarily follow, that the *momenta* of equal bodies will be as the velocities, or as the squares of the velocities respectively; it being certain, that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumstances that attend the application of it, the same conclusion, in point of computation, will result. I should not, therefore, have thought it worth while to trouble the Society upon this subject, had I not found, that not only myself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying these doctrines to practical mechanics, by sometimes forgetting or neglecting

neglecting the due regard which ought to be had to these collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which often require very great sums of money in their execution. I shall mention the following instances.

DESAGULIERS, in his second volume of Experimental Philosophy, treating upon the question concerning the forces of bodies in motion, after taking much pains to shew that the dispute, which had then subsisted fifty years, was a dispute about the meaning of words; and that the same conclusion will be brought out, when things are rightly understood, either upon the old or new opinion, as he distinguishes them; among other things, tells us, that the old and new opinion may be easily reconciled in this instance: that the wheel of an undershot water-mill is capable of doing quadruple work when the velocity of the water is doubled, instead of double work only; "because (the adjutage being the same), says he, we find, that "as the water's velocity is double, there are twice the number "of particles of water that issue out, and therefore the ladle-"board is struck by twice the matter, which matter moving "with twice the velocity that it had in the first case, the whole "effect must be quadruple, though the instantaneous stroke of "each particle is increased only in a simple proportion of the "velocity." See vol. II. Annotations on lecture 6th, p. 92.

Again, in the same volume, lecture 12th, p. 424, referring to what went before, he tells us, "The knowledge of the fore-"going particulars is absolutely necessary for setting an under-"shot wheel to work; but the advantage to be reaped from it "would be still guess work, and we should be still at a loss to "find out the utmost it can perform, if we had not an inge-"nious proposition of that excellent mechanic M. PARENT,

"of

"of the Royal Academy of Sciences, who has given us a "*maximum* in this case, by shewing, that an undershot wheel "can do the most work, when its velocity is equal to the third "part of the velocity of the water that drives it, &c. because "then two-thirds of the water is employed in driving the wheel "with a force proportionable to the square of its velocity. If "we multiply the surface of the adjutage or opening by the "height of the water, we shall have the column of water that "moves the wheel. The wheel thus moved will sustain on "the opposite side only four-ninths of that weight, which will "keep it in equilibrio; but what it can move with the velocity "it goes with, will be but one-third of that weight of equili-"brium; that is, $\frac{4}{27}$ths of the weight of the first column, &c.— "This is the utmost that can be expected."

The same conclusion is likewise adopted by MACLAURIN, in art. 907. p. 728. of his Fluxions, where, giving the fluxionary deduction of M. PARENT'S proposition, he says, "that if A re-"presents the weight which would balance the force of the "stream, when its velocity is $a$; and $u$ represents the velocity "of the part of the engine, which it strikes when the motion "of the machine is uniform, &c.—the machine will have the "greatest effect when $u$ is equal to $\frac{a}{3}$; that is, if the weight "that is raised by the engine be less than the weight which "would balance the power, in the proportion of 4 to 9, and the "*momentum* of the weight is $\frac{4Aa}{27}$."

Finding that these conclusions were far from the truth; and seeing, from many other circumstances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of consulting *; in

* BELIDOR, *Architecture Hydraulique*, greatly prefers the application of water to an undershot mill, instead of an overshot; and attempts

in the year 1751 I began a course of experiments upon this subject. These experiments, with the conclusions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Transactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy President the late Earl of MACCLESFIELD. Those experiments and conclusions stand uncontroverted, so far as I know to this day; and having since that time been concerned in directing the construction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between these two, appears to me fully to establish the truth of the

attempts to demonstrate, that water applied undershot, will do six times more execution than the same applied overshot. See vol. I. p. 286. While DESAGULIERS, endeavouring to invalidate what had been advanced by BELIDOR, and greatly preferring an overshot to an undershot, says, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, "a well-made overshot mill ground as "much corn in the same time with ten times less water;" so that betwixt BELIDOR and DESAGULIERS, here is a difference of no less than 60 to 1.

Again, BELIDOR, vol. II. p. 72. says, that the centre of gravity of each sail of a windmill should travel in its own circle with one-third of the velocity of the wind; so that, taking the distance of this centre of gravity from the centre of motion at 20 feet, as he states it p. 38. art. 849. the circumference will be exceeding 126 feet English measure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a fresh wind and all their cloth spread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greatest storms we experience in this climate.

princi-

principles upon which they were constructed, when applied to great works, as well as upon a smaller scale in models.

Respecting the explanatory deduction of DESAGULIERS in the first example above-mentioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is shown, in my former Essay, p. 21, 22, and 24, part I. maxim 4, that, where the velocity of water is double, the adjutage or aperture of the sluice remaining the same, the effect is eight times; that is, not as the square but as the cube of the velocity; and the same is investigated concerning the power of the wind arising from difference of velocity, p. 52. being part 3. maxim 4.

The conclusion in the second example above-mentioned, adopted both by DESAGULIERS and MACLAURIN, is not less wide of the truth than the foregoing; for if that conclusion were true, only $\frac{4}{27}$ths of the water expended could be raised back again to the height of the reservoir from which it had descended, exclusively of all kinds of friction, &c. which would make the actual quantity raised back again still less; that is, less than one-seventh of the whole; whereas it appears, from Table I. of the preceding essay, that in some of the experiments there related, even upon the small scale on which they were tried, the work done was equivalent to raising back again about one quarter of the water expended; and in large works the effect is still greater, approaching towards half, which seems to be the limit for the undershot mills, as the whole would be the limit for the overshot mills, if it were possible to set aside all friction, resistance from the air, &c. see p. 29.

The velocity also of the wheel, which, according to M. PARENT'S determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of the water, varies at the *maximum* in the above-mentioned experiments of

Table

Table I. between one-third and one-half; but in all the cases there related, in which the most work is performed in proportion to the water expended, and which approach the nearest to the circumstances of great works, when properly executed, the *maximum* lies much nearer to one-half than one-third; one-half seeming to be the true *maximum*, if nothing were lost by the resistance of the air, the scattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminish the effect more, at what would be the *maximum* if these did not take place, than they do when the motion is a little slower.

Finding these matters, as well as others, to come out in the experiments, so very different from the opinions and calculations of authors of the first reputation, who, reasoning according to the Newtonian definition, must have been led into these errors from a want of attending to the proper collateral circumstances; I thought it very material, especially for the practical artist, that he should make use of a kind of reasoning in which he should not be so liable to mistakes; in order, therefore, to make this matter perfectly clear to myself, and possibly so to others, I resolved to try a set of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the same body different degrees of velocity. This scheme was put in execution in the year 1759, and the experiments were then shewn to several friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 6. part I. defined what I meant by power as applied to practical mechanics, that is, what I now call mechanical power; which, in terms synonymous to those there used, may be said to be measured by multiplying the weight of the body into the perpendicular height from

which

which it can descend; thus the same weight, descending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight descending from the same height is also a double power, because it likewise is capable of producing a double effect; and a given body, descending through a given perpendicular height, is the same power as a double body descending through half that perpendicular; for, by the intervention of proper levers, they will counterbalance one another, conformably to the known laws of mechanics, which have never been controverted. It must, however, he always understood, that the descending body, when acting as a measure of power, is supposed to descend slowly, like the weight of a clock or a jack; for, if quickly descending, it is sensibly compounded with another law, *viz.* the law of acceleration by gravity.

## DESCRIPTION OF THE MACHINE.

A B is the base of the machine placed upon a table.

A C is a pillar or standard.

C D is an arm, upon the extremity of which is fixed a plate *f g*, which is here seen edge-ways, through which is a small hole for receiving a small steel pivot *e*, fixed in the top of the upright axis *e* B; the lower end of this axis finishes in a conical steel point, which rests upon a small cup of hard steel polished at B.

H I is a cylinder of white fir, which passes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of sliding.

K L two cylindric weights of lead of equal size, which are capable of being fixed upon any part of the cylindric arms,

from

from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal distances from the centre, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed M N, two cylindrical barrels, where of M is double the diameter of N; they have a little pin stuck into one side of each at *o*, *p*.

Q is a piece capable of sliding higher or lower, as occasion requires; and carries

R, a light pulley of about three inches diameter, hung upon a steel axis, and moveable upon two small pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one side, so as to point (at a mean) between the surface of the bigger barrel and the less.

S is a light scale for receiving weights, and hangs by a small twine, cord, or line, that passes the pulley, and terminates either upon the bigger barrel or the less, as may be required; the sliding-piece Q being moved higher or lower for each, that the line, in passing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furthest from the scale, is terminated by a small loop, which hangs on upon the pin *o*, or the pin *p*, according as the bigger or the lesser barrel is to be used.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the scale S, it is obvious, that it will cause the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulse of the weight in the scale; and when the line is wound off to the

pin,

pin, the loop slips off, and the scale then falling down, the weight will cease to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except so far as it must be gradually lessened by the friction of the machine and resistance of the air, which being small, the bodies will revolve sometime before their velocity is apparently diminished.

## MEASURES OF SOME PARTS OF THE MACHINE.

| | Inches. |
|---|---|
| Diameter of the cylinders of lead, or the heavy bodies | 2,57 |
| Length of ditto | 1,56 |
| Diameter of the hole therein | ,72 |

Weight of each cylinder 3 lbs. avoirdupois.

| | |
|---|---|
| Greater distance of the middle of each body from the centre of the axis | 8,25 |
| The smaller distance of ditto | 3,92 |
| 10 turns of the smaller barrel raises the scale, 5 ditto of the bigger ditto | 25,25 |

When the bodies are at the smaller distance above specified, from the axis of rotation, they are then in effect at half the greater distance from that axis: for, since the axis itself, and the cylindric arms of wood, keep an unvaried distance from the centre of rotation, the bodies themselves must be moved nearer than half their former distance, in order that, compounded with the unvariable parts, they may be virtually at the half distance. In order to find this half distance nearly, I put in an arm of the same wood, that only went through the axis, without extending in the opposite direction; one of the bodies being put upon the end of this arm, at the distance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum,

dulum, and vibrated at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the same inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches distance from the centre, as above stated, which is about $\frac{2}{10}$ths of an inch nearer than the half distance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjusted ready for use; and with it were tried the following experiments, each of which was repeated so many times as to be fully satisfactory.

TABLE OF EXPERIMENTS.

| N°. | Ounces Avoirdupois in the Scale. | Barrel used, M the bigger, N the smaller. | The Arms, W the whole, H the half length. | Number of Turns of the line wound on the Barrel. | Time of the Descent of the Weight in the Scale. | Time in making 20 Revolutions with equable motions. |
|---|---|---|---|---|---|---|
| | | | | | ″ | ′ |
| 1 | 8 | M | W | 5 | $14\frac{1}{4}$ | 29 |
| 2 | 8 | N | W | 10 | $28\frac{1}{4}$ | $29\frac{1}{4}$ |
| 3 | 8 | N | W | $2\frac{1}{2}$ | $14\frac{1}{4}$ | $58\frac{1}{2}$ |
| 4 | 32 | M | W | 5 | 7 | 14 |
| 5 | 32 | N | W | 10 | 14 | $14\frac{3}{4}$ |
| 6 | 32 | N | W | $2\frac{1}{2}$ | 7 | $28\frac{3}{4}$ |
| 7 | 8 | M | H | 5 | 7 | $14\frac{3}{4}$ |
| 8 | 8 | N | H | 10 | 14 | 15 |
| 9 | 8 | N | H | $2\frac{1}{2}$ | 7 | $30\frac{1}{4}$ |
| 1 | 2 | 3 | 4 | 5 | 6 | 7 |

The $58''\frac{1}{2}$ in number 3, column 7, was determined in fact from $29''\frac{1}{4}$, being the time of making 10 equable revolutions after the weight was dropped off, in order to prevent the sensible retardation that might take place, and affect the observation, if continued for twenty revolutions made so slowly.

## FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be necessary also to define the following terms:

Impulse or Impulsion,
Impulsive Force or Power,
Impelling Force or Power,
} By all which, I understand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be measured either by its being a weight of itself, or by being counter-balanced by a weight. It may also act either immediately upon the body to be moved, so that if motion is the consequence, they move with the same velocity; and that, either by a simple contact, or by being drawn as by a cord, or pushed as by a staff: or it may act by the intervention of a lever or other mechanic instrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers must be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a standard length of lever, which is the method taken in the subsequent reasoning upon the preceding experiments. An impelling power, therefore, consisting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intensity.

OBSER-

## OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

1st, By the first experiment it appears, that the mechanic power employed, consisting of 8 ounces in the scale, deliberately descending (by 5 turns of the bigger barrel (through a perpendicular space $25\frac{1}{4}$ inches, will represent the quantity of mechanic power which causes the two heavy bodies, from a state of rest, to acquire a velocity, such as to carry them equably through 20 circumferences of their circle of revolution in the space of 29″; and that the time in which the mechanic power produced this effect was $14''\frac{1}{4}$, as appears by column 6th. And this mechanic power we shall express by the number 202, the product of the number of ounces in the scale multiplied by the inches in its perpendicular descent, for $8 \times 25\frac{1}{4} = 202$.

2d, By the second experiment, as 10 turns of the smaller barrel are equal to the same perpendicular height as 5 turns of the bigger, it follows, that the same mechanic power, *viz.* 202, acting upon the same heavy bodies to accelerate them, produces the very same effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in $29''\frac{1}{4}$, the small difference of $\frac{1}{4}$ of a second being no more than may reasonably be attributed to the unavoidable errors arising from friction of the machine, want of perfect accuracy in its measures, resistance of the air, and imperfections in the observations themselves, which must not only be allowed for in this, but the rest; but as the impelling power is acting here upon a lever of but half the length, and, consequently, but half the intensity, when referred to the bodies to be moved, it takes just double the time to generate the same velocity therein.

DEDUCTION. It appears from hence, that the same mechanic power is capable of producing the same velosity in a gi-

ven body, whether it is applied so as to produce it in a greater or a lesser time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a simple inverse proportion of the intensity of the impulsive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the lesser barrel, the same weight in the scale of 8 ounces descending only 1-quarter part of the former perpendicular, the mechanic power employedwill be onlyone quarter part of the former, *viz.* $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58''\frac{1}{2}$; that is, nearly 10 revolutions in 29″; we may conclude, in this instance, that the mechanic power, employed in producing motion, is as the square of the velocity produced in the same body; and that the velocity produced is as the time that an impelling power, of the same intensity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the same as the first, only here the weight in the scale is 32 ounces; that is, the impelling power is the quadruple of the first, and hereby a double velocity is given to the bodies; for they make 20 revolutions in 14″, which is a small matter less than half the time taken up in making 20 revolutions in the first experiment. It also appears, that the velocity acquired is simply as the impelling power compounded with the time of its action; for a quadruple impulsion acting for 7″ instead of 14″ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the first, it holds here also, that the mechanic power, to be necessarily employed, is as the square of the velocity to be generated; that is, in the same proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the first.

5thly

5thly, The fifth and sixth experiments were made with a mechanic power four times greater than those employed in numbers 2 and 3 respectively; and since the same deductions result from hence as from numbers 2 and 3, they are additional confirmations of the conclusions drawn from them and from the last article.

6thly, In the seventh experiment, the disposition of the apparatus is the same as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the same mechanic power still produces the same velocity in the same bodies; for though 20 revolutions were performed in 14″ $\frac{3}{4}$ (see column 7) which is nearly half the time that 20 revolutions were performed in the first experiment; yet, since the circles in which the bodies revolved in the seventh are only of half the circumference as those of number 1, it is obvious, that the absolute velocity acquired by the moving bodies in the two cases is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithstanding, this will coincide with the former conclusions, if the intensity of the impelling power is compounded therewith; for, though the barrel was the same with the same number of turns as in number 1, and therefore the lever the same, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the centre, the impelling power acting by the first lever, would act upon the second with double the intensity, according to the known laws of mechanics; that is, it would require a double weight opposing the bodies, to prevent their moving, in order to balance it. An impulsive power, therefore, of double the intensity, acting for half the time, produces the same effect in generating motion, as an impulsive power, of half the intensity, acting for the whole time.

7thly, The eighth and ninth experiments afford the same deductions and confirmations relative to the seventh experiment, that

that the fifth and sixth do respecting the fourth, and that the second and third do respecting the first; and from the near agreement of the whole, when the necessary allowances before mentioned are made, together with some small inequality arising from the mechanical power lost by the difference of the motion given by gravity to the weight in the scale: I say, from these agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may safely conclude, that this is the universal law of nature, respecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power necessary to be employed to produce or generate different velocities (the bodies being supposed equal in their quantity of matter); that the mechanic powers to be expended are as the squares of the velocities to be generated, and *vice versâ*; and that the simple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice versâ*.

We shall, perhaps, form a still clearer conception of the relation between velocities produced, and the quantity of mechanic power required to produce them; together with the collateral circumstances attending, by which these propositions, seemingly two, are reconciled and united, by stating the following popular elucidation, which indeed was the original idea that occurred to me on considering this subject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppose then a large iron ball of 10 feet diameter, turned truly spherical, and set upon an extended plane of the same metal, and truly level. Now, if a man begins to push at it, he will find it very resisting to motion at first; but, by continuing the impulse, he will gradually get it into motion, and having nothing to resist it but the air, he will, by continuing his efforts, at length get it to roll almost as fast as he can run.

Sup-

Suppose now, in the first minute he gets it rolled through a space of one yard; by this motion, proceeding from rest (similar to what happens to falling bodies,) it would continue to roll forward at the rate of two yards *per* minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have have given it a velocity capable of carrying it through a space of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increase of velocity, and made it proceed at the rate of six yards *per* minute; and so on, increasing its velocity at the rate of two yards in every minute. The man therefore, in the space of every minute exerts an equal impulse upon the ball, and generates an equal increase of movement correspondent to the definition of Sir ISAAC NEWTON. But let us see what happens besides: the man, in the first minute, has moved but one yard from where he set out; but he must in the second minute move two yards more, in order to keep up with the ball; and as he exerted an impulse upon it, so as at the end of the second minute to have given it an additional velocity of the two yards, he must also in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the space, that he will of consequence have actually been obliged to go through in the second minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the second minute; so that being one yard from his original place at the beginning of the second minute, at the end of it he will have moved the sum of the journies of the first and second minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he must travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; so that in the third minute, he must travel five yards to keep up the same

impelling

impelling power upon the ball that he did in the first minute in travelling one, so that these five yards in the third minute added to the four yards that he had travelled in the two preceding minutes, sets him at the end of the third minute nine yards from whence he set out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of six yards *per* minute, as before stated. We may now leave the further pursuit of these proportions, and see how the account stands. He generated a velocity of two yards *per* minute in the first minute, the square of which is four, when he had moved but one yard from his place; and he had generated a velocity of six yards *per* minute, the square of which is thirty-six, at the end of the third minute, when he had travelled nine yards from his place. Now, since the square of the velocity, generated at the end of the first minute, is to that of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and since the spaces, moved through by the man to communicate these velocities, are also as 1 : 9, it follows, that the spaces through which the man must travel, in order to generate these velocities respectively (preserving the impelling power perfectly equal), must be as the squares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally resisting, this would be the measure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that must of necessity be employed in giving different degrees of velocity to the same body, must be as the square of that velocity; and if the converse of this proportion did not hold, *viz.* that if a body in motion, in being stopped, would not produce a mechanical effect equal or proportional to the square of its velocity, or to the mechanical power employed in producing it, the effect would not correspond with its producing cause.

Thus

Thus the consequences of generating motion upon a level plane exactly correspond with the generating of motion by gravity; *viz*, that though in two seconds of time the equal impulsive power of gravity gives twice the velocity to a body that it does in one second, yet this collateral circumstance attends it, that at the end of the double time, in consequence of the velocity acquired in the first half, the body has fallen from where it set forward through four times the perpendicular; and therefore, though the velocity is only doubled, yet four times the mechanical power had been consumed in producing it, as four times the mechanical power must be expended in bringing up the fallen body to its first place.

This then appears to be the foundation, not only of the disputes that have arisen, but of the mistakes that have been made, in the application of the different definitions of quantity of motion; that while those, that have adhered to the definition of SIR ISAAC NEWTON, have complained of their adversaries, in not considering the time in which effects are produced, they themselves have not always taken into the account the space that the impelling power is obliged to travel through, in producing the different degrees of velocity. It seems, therefore, that, without taking in the collateral circumstances, both of time and space, the terms, quantity of motion, *momentum*, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, *viz*. mechanic power.

From the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that, whatever mechanical effect is found to be produced in a given time,

time, the uniform continuance of the action of the same mechanical power will, in a double time, produce two such effects, or twice that effect. A mechanical power, therefore, properly speaking, is measured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a lesser time; thus, having treasured up 1000 tuns of water, which I can let out upon the overshot wheel of a mill, and descending through a perpendicular of 20 feet, this power applied to proper mechanic instruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But suppose the mill equally adapted to produce a proportionable effect, by the application of a greater impulsive power as with a less; then, if I let out the water twice as fast upon the wheel, it will grind the corn twice as fast, and both the water will be expended and the corn ground in half an hour. Here the same mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the same mechanical power, *viz.* 1000 tuns of water descending through a given perpendicular of 20 feet, and yet this effect is in one case produced in half the time of the other. What time, therefore, has to do in the business is this: let the rate of doing the business, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the same rate, twice the effect will be produced in twice the time, on supposition that I have a supply of mechanic power to go on with. Thus 1000 tuns of water, descending through 20 feet of perpendicular, being, as has been shewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I must wait another hour before it be renewed, by the natural flow of a river, or otherwise, I can then only expend twelve such quantities of power in 24 hours; but if, while I am expending 1000 tuns

tuns in one hour, the stream renews me the same quantity, then I can expend 24 such quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common measure; but the quantity of mechanic power arising from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

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NEW

# FUNDAMENTAL EXPERIMENTS

UPON THE

# COLLISION OF BODIES.

By Mr. JOHN SMEATON, F. R. S.

# EXPERIMENTS

UPON THE

# COLLISION OF BODIES.

*Read before the Royal Society, April* 18, 1782.

IT is universally acknowledged, that the first simple principles of science cannot be too critically examined, in order to their being firmly established; more especially those which relate to the practical and operative parts of mechanics, upon which much of the active business of mankind depends. A sentiment of this kind occasioned my tract upon *Mechanic Power* which is the second paper of this Volume. What I have now to offer was intended as a supplement thereto, and the experiments were then, in part tried; but the completion thereof was deferred at that time, partly from want of leisure; partly to avoid too great a length of the paper itself; and partly to avoid the bringing forward too many points at once.

My present purpose is to shew, that the true doctrine of the *collision of bodies* hangs as it were upon the same hook, as the doctrine of the gradual generation of motion from rest, considered in that paper: that is, that whether bodies are put into

gradual

gradual motion, and uniformly accelerated from rest to any given velocity; or are put in motion, in an instantaneous manner, when bodies of any kind strike one another; the motion, or sum of the motions produced, has the same relation to mechanic power therein defined, which is necessary to produce the motion desired. To prove this, and at the same time to show some capital mistakes in principle, which have been *assumed* as indisputable truths by men of great learning, is the reason of my now pursuing the same subject.

I do not mean to point out the particular mistakes which have been made by particular men, as that would lead me into too great a length: I shall therefore content myself with observing, that the laws of collision, which have been investigated by mathematical philosophers, are principally of three kinds; *viz.* those relating to bodies perfectly *elastic*; to bodies perfectly unelastic, and perfectly *soft*; and to bodies perfectly unelastic, and perfectly *hard*. To avoid prolixity, I shall consider in each, only the simple case of two bodies which are equal in weight or quantity of matter striking one another. Respecting those which are perfectly elastic, it is universally agreed, that when two such bodies strike one another, no motion is lost; but that in all cases, what is lost by one is acquired by the other: and hence, that if an elastic body in motion strikes another at rest, upon the stroke the former will be reduced to a state of rest, and the latter will fly off with an equal velocity.

In like manner, if a non-elastic *soft* body strikes another at rest, they neither of them remain at rest, but proceed together from the point of collision with exactly one half of the velocity that the first had before the stroke; this is also universally allowed to be true, and is fully proved by every good experiment upon the subject.

Respecting the third species of body, that is, those that are non-elastic and yet perfectly hard: the laws of motion relating

to

to them, as laid down by one species of philosophers, have been rejected by another; the latter alledging, that there are no such bodies to be found in nature whereon to try the experiment; but those who have laid down and assigned the doctrine that would attend the collision of bodies, of this kind, (if they could be found) have universally agreed, that if a non-elastic *hard* body was to strike another of the same kind at rest, that, in the same manner as is agreed concerning non-elastic soft bodies, they neither of them would remain at rest, but would in like manner proceed from the point of collision, with exactly one half of the velocity that the first had before the stroke: in short, they lay it down as a rule attending all non-elastic bodies, whether hard or soft, that the velocity after the stroke will be the same in both, *viz*, *one half* of the velocity of the original striking body.

Here is therefore the assumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reason that I know of, *viz.* that the velocity of non-elastic *hard* bodies after the stroke must be the same as that resulting from the stroke of non-elastic *soft* bodies; and the question now is, whether it is true or not?

Here it may be very properly asked, what ill effects can result to practical men, if philosophers should reason wrong concerning the effects of what does not exist in nature, since the practical men can have no such materials to work upon, or misjudge of? But it is answered, that they who infer an equality of effects between the two sorts, may from thence be misled themselves, and in consequence mislead practical men in their reasonings and conclusions concerning the sort with which they have abundant concern, to wit, the non-elastic *soft bodies*, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills, I never had doubted the truth of the doctrine, that the same velocity resulted



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from the stroke of both sorts of non-elastic bodies; but the trial of those experiments made me clearly see at least the inconclusiveness, if not the falsity of that doctrine: because I found a result which I did not expect to have arisen from either sort; and for the which, when it appeared from experiment, I could see a substantial reason why it should take place in one sort, and that it was impossible that it could take place in the other; for if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypothesis: of this deduction I have given notice in my said tract on mills, p. 30. *The effect therefore of overshot wheels, &c.*

It may also be said, that since we have no bodies perfectly elastic, or perfectly unelastic and *soft*, why should we expect bodies perfectly unelastic and *hard?* Why may not the effects be such as should result from a supposition of their being *imperfectly elastic* joined with their being *imperfectly hard?* But here I must observe, that the supposition appears to be a contradiction in terms.

We have bodies which are so nearly perfectly elastic, that the laws may be very well deduced and confirmed by them; and the same obtains with respect to non-elastic *soft* bodies; but concerning bodies of a mixed nature, which are by far the greatest number, so far as they are wanting in elasticity, they *are soft* and *bruise, yield,* or *leave a mark* in collision; and so far as they are not perfectly soft they are elastic, and observe a mixture of the law relative to each; but imperfectly elastic bodies, imperfectly hard come in reality under the *same description* as the former mixed bodies; for so far as they are imperfectly hard they are soft, and either *bruise* and *yield,* or leave a mark in the stroke; and so far as they want perfect elasticity, they are non-elastic; that is to say, they are bodies imperfectly elastic, and imperfectly soft; and in fact I have never yet seen any bodies but what come under this description. It seems, therefore, that respecting the *hardness* of bodies they differ in degrees

grees of it, in proportion as they have a greater degree of tenacity or cohesion; that is, are further removed from perfect softness, at the same time that their elastic springs, so far as they reach, are very stiff; and hence we may (by the way) conclude, that the same mechanic power that is required to change the figure in a *small degree* of those bodies that have the popular appellation of *hard bodies*, would change it in a *great degrees* in those bodies that approach towards softness, by having a small degree of tenacity or cohesion. In the former kind we may rank the harder kinds of *cast iron*, and in the latter, *soft tempered clay*.

While the philosophical world was divided by the dispute about the *old* and *new opinion*, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities: those who held the old opinion contending, that it was as the velocity *simply*, asked those of the new, How, upon their principles, they would get rid of the conclusions arising from the doctrine of unelastic perfectly hard bodies? They replied, they found no such bodies in nature, and therefore did not concern themselves about them. On the other hand, those of the new opinion asked those of the old, How they would account for the case of non-elastic soft bodies, where, according to them, the whole motion lost by the striking body was retained in the two after the stroke (the two bodies moving together with the half velocity,) though the two non-elastic bodies had been bruised and changed their figure by the stroke; for, if no motion was lost, the change of figure must be an effect without a cause? To obviate this, those of the old opinion seriously set about proving, that the bodies might change their figure, without any loss of motion in either of the striking bodies.

Neither of these answers have appeared to me satisfactory, especially since my mill experiments: for with respect to the first, it is no proper argument to urge the impossibility of find-

ing the proper material for an experiment, in answer to a conclusion drawn from an abstract idea. On the other hand, if it can be shewn, that the figure of a body can be changed, without a *power*, then, by the same law, we might be able to make a *forge hammer* work upon a moss of soft iron, without any other power than that necessary to overcome the friction: resistance, and original *vis inertiæ*, of the parts of the machine to be put in motion: for, as no progressive motion is given the mass of iron by the hammer (it being supported by the anvil, no power can be expended that way; and if none is lost to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power must reside in the hammer, and it would jump back again to the place from which it fell, just in the same manner as if it fell upon a body perfectly elastic, upon which, if it did fall, the case would really happen: the power therefore, to work the hammer would be the same, whether it fell upon an elastic or non-elastic body; an idea so very contrary to all experience, and even apprehension, of both the philosopher and the vulgar artist, that I shall here leave it to its own condemnation.

As nothing, however, is so convincing to the mind as experiments obvious to the senses, I was very desirous of contriving an experiment in point, and as I saw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one: so circumscribed, however, as to prove incontestably, that the result of the stroke of two non-elastic perfectly hard bodies could not be the same as would result from the collision of two soft ones; that is, if it can be *bona fide* proved, that *one-half* of the original power is lost in the stroke of soft bodies by the change of figure (as was very strongly suggested by the mill experiments); then since no such loss can happen in the collision of bodies perfectly hard, the result and consequence of such a stroke must be *different*

The consequence of a stroke of bodies perfectly hard, but void of elasticity, must doubtless be different from that of bodies

perfectly

perfectly *elastic:* for having no spring the body at rest could not be driven off with the velocity of the striking body, for that is the consequence of the action of the spring or elastic parts between them, as will be shewn in the result of the experiments; the striking body will therefore not be stopped, and as the motion it loses must be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity, as in the case of non-elastic soft bodies: the question, therefore, that remains is, what that *velocity must be?* It must be greater than that of the non-elastic soft bodies, because there is no mechanical power lost in the stroke. It must be less than that of the striking body, because, if equal, instead of a loss of motion by the collision it will be doubled. If, therefore, non-elastic soft bodies lose half their motion, or mechanical power, by change of figure in collision, and yet proceed together with half the velocity, and the non-elastic hard bodies can lose *none* in any manner whatever; then, as they must move together, their velocity must be such as to preserve the equality of the mechanic power, *unimpaired*, after the stroke the same as it was before it.

For example, let the velocity of the striking body before the stroke be 20, and its mass or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on *Mechanical Power* (see exp. third and fourth) that power will be expressed by $20 \times 20 = 400$, which $\times 8 = 3200$; and if half of it is lost in the stroke, in the case of non-elastic soft bodies, it will be reduced to 1600; which $\div 16$ the double quantity of matter, will give 100 for the square of their velocity; the square root of which being 10, will be the velocity of the two non-elastic soft bodies after the stroke, being just one half of the original velocity, as it is constantly found to be. But in the non-elastic hard bodies, no power being lost in the stroke, the mechanic power will remain after it, as before it, $= 3200$; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the square of the velocity, the square root

root of which is 14. 14, &c. for their velocity after the stroke, which is to 10, the velocity of the non-elastic soft bodies after the stroke, as the square root of 2 to 1, or as the diagonal of a square to its side.

It remains, therefore, now to be proved, that precisely half of the mechanic power is *lost* in the collision of non-elastic soft bodies; for which purpose my mind suggested the following reflections. In the collision of elastic bodies the effect seemingly instantaneous, is yet performed in *time;* during which time the natural springs residing in elastic bodies, and which constitute them such, are bent or forced, till the motion of the striking body is divided between itself and the body at rest; and in this state the two bodies would then proceed together, as in the case of non-elastic soft bodies; but as the springs will immediately restore themselves in an equal time, and with the same degree of *impulsive force*, wherewith they were bent in this re-action, the motion that remained in the striking body will be totally destroyed, and the total exertion of the two springs, communicated to the original resting body, will cause it to fly off with the same velocity wherewith it was struck.

Upon this idea, if we could construct a couple of bodies in such a way that they should either act as bodies perfectly elastic; or that their springs should at pleasure be hooked up, retained, or prevented from restoring themselves, when at their extreme degree of bending; and if the bodies under these circumstances observed the laws of collision of non-elastic soft bodies, then it would be proved, that one half of the mechanical power, residing in the striking body, would be lost in the action of collision; because the impulsive force or power of the spring in its restitution being cut off, or suspended from acting, which is equal to the impulsive force or power to bend it (and which alone has been employed to communicate motion from one body to the other), it would make it evident, that one half of the impulsive force is lost in the action, as the other half remains *locked up* in the

the springs, it also follows, as a *collateral circumstance*, that be the impulsive power of the springs what it may from first to last, yet as one half of the *time* of the action is by this means cut off, in this sense also it will follow, that one half of the mechanic power is destroyed; or rather, in this case, remains locked up in the springs, capable of being *re-exerted* whenever they are set at liberty, and of producing a fresh mechanical effect, equivalent to the motion or mechanical power of the two non-elastic soft bodies after their collision.

Hence we must infer, that the quantity of mechanical power expended in displacing the parts of non-elastic soft bodies in collision, is exactly the same as that expended in bending the springs of perfectly elastic bodies; but the difference in the ultimate effect is, that in the non-elastic soft bodies, the power taken to displace the parts will be totally lost and destroyed, as it would require an equal mechanic power to be raised a-fresh, and exerted in a contrary direction to restore the parts back again to their former places; whereas, in the case of the elastic bodies, the operation of half the mechanic power is, as observed already, only locked up and suspended, and capable of being re-exerted without a further original accession.

These ideas arose from the result of the experiments tried upon the machine described in my said tract upon mechanic power, and were also communicated to my very worthy and ingenious friend WILLIAM RUSSEL, Esq. F. R. S. at the same time that I shewed him those experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has since occurred; and though some rough trials, sufficient to shew the effect, were made thereon, prior to the offering the Paper on Mechanical Power to the Society in 1776, yet the machine itself I had not leisure to compleat to my satisfaction till lately; which I mention to apologize for the length of time that these speculations have taken in bringing forward.

DESCRIPTION

## DESCRIPTION OF THE MACHINE FOR COLLISION.

FIG. I. shews the front of the Machine as it appears at rest when fitted for use.

A is the pedestal, and A B the pillar which supports the whole, C D are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. These bodies are alike in construction, which will be more particularly explained by fig 2. These bodies are suspended by two white fir rods of about half an inch diameter, *e f* and *g h* being about four feet long from the point of suspension to the centre of the bodies; and their suspension is upon the cross piece II, which is mortised through, to let the rods pass with perfect freedom; and they hang upon two small plates filed to an edge on the under side, and pass through the upper part of the rods. Their centres are at *k* and *l*, and the edges being let into a little notch, on each side the mortoise, the rods are at liberty to vibrate freely upon their respective points (or rather edges) of suspension, and are determined to one plain or vibration. M N is a flat arch of white wood, which may be covered with paper, that the marks thereupon may be more conspicuous.

The cross piece II is made to project so far before the pillar, that the bodies in their vibrations may pass clear of it, without danger of striking it; and also the arch M N is brought so far forward as to leave no more than a clearance, sufficient for the rods to vibrate freely without touching it.

FIG. 2. shews one of the compound bodies, drawn of its full size. A B is a block of wood, and about as much in breadth as it is represented in height, through a hole in which the wood rod C C passes, and is fixed therein.

D B represents a plate of lead about three-eighths of an inch thick, one on each side, screwed on by way of giving it a competent weight. *d* B *e f g* represents the edge of a springing place of brass, rendered elastic by hard hammering; it is about five eighths of an inch in breadth, and about one twentieth of an inch thick. It is fixed down upon the wooden block at its end *d* B by means of a bridge plate, whose end is shewn *h i*, and is screwed down on each side of the spring plate by screws, which being relaxed the spring can be taken out at pleasure, and adjusted to its proper situation. *k l* is a light thin slip of a plate, whose under edge is cut into teeth like a fine saw or rachet, and is attached to the spring by a pin at *k*, which passes through it, and also through a small stud rivetted into the back part of the spring, and upon which pin, as a center, it is freely moveable. *m n* shews a small plate or stud seen edgeways raised upon the bridge plate, through an hole in which stud the ratchet passes; and the lower part of the hole is cut to a tooth shaped properly to catch the teeth of the ratchet, and retain it together with the spring at any degree to which it may be suddenly bent; and for this intent it is kept bearing gently downward, by means of a wire spring *o p q*, which is in reality double, the bearing part at *o* being semi-circular; from which branching off on each side the rod C C, passes to *p*, and fixes at each end into the wood at *q*. However to clear the ratchet, which is necessarily in the middle as well as the rod, the latter is perforated; and also the block is cut away, so far as to set the mainspring at *e* free of all obstacles that would prevent its play from the point B. The part *f g* is shewn thicker than the rest, by being covered with thin kid leather tight sewed on, to prevent a certain jarring that otherwise takes place on the meeting of the springs in collision.

Let us now return to *fig.* 1. the marks upon the arch M N are put on as follows. *o p* is an arch of a circle from the center *l*, and *q r* an arch of a circle from the center *k*, intersecting each other at S. Now the middle line of the marks *t*, *v*, are at the same distance from the middle line at S that the centers *k l* are;

are; so that when each body hangs in its own free position, without bearing against the other, the rod *e f* will cover the mark at *t*, and the rod *g h* will cover the mark at *v*. From the point S upon the arches S *p* and S *q* respectively, set off points at an equal and competent distance from S each way, which will give the middle of the mark *w* and *x*: and upon the arch S *p* find a middle point between the mark *v* and *w*, which let be *y*; and on the other side, in like manner, upon the arch S *q* find a middle point for the mark *z*; then set off the distance S *v* or S *t* from *y* each way, and from *z* each way; and from these points, drawing lines to the respective center *l* and *k*, they will give the place and position of the marks *a*, *b*, and *c*, *d*; and thus is the machine prepared for use.

## FOR TRIALS ON ELASTIC BODIES.

FOR this use take out the pins and ratchets from each respectively, and the springs being then at liberty, with a short bit of stick (suppose the same size as the rods) turn aside the rod *g h* with the right hand, carrying the body D upwards till the stick is upon the mark *w*, as suppose at o; there hold it, and with the left set the body C perfectly at rest; in which case the rod *e f* will be over the mark *t*; then suddenly withdraw the stick, in the direction that the rod *g h* is to follow it, and the spring of the body D, impinging upon that of the body C, they will be both bent, and also restored; and the body C will fly off, and mount till its rod *e f* covers the mark *x*; the rod of the striking body D remaining at rest upon its proper mark of rest *v*, till the body C returns, when the body D will fly off in the same manner; the two bodies thus rebounding a number of times, losing a part of their vibration each time; but so nearly is the theory of elastic bodies fulfilled hereby, that the single advantage of originally pushing the rod *g h* beyond the mark *w*, by the thickness of the stick, or its own thickness, is sufficient to carry the rod of the quiescent body C completely to its mark *x*.

There

There are several other experiments which may be made with this apparatus, in confirmation of the doctrine of the collision of elastic bodies; which being universally agreed upon, and well known, it is needless further to dwell upon here; but respecting the application to non-elastic soft bodies, it is far more difficult to come at a fitness of materials for this kind of experiment, than it is for those supposing perfectly elasticity. The conclusions, however, may be attained with equal certainty.

## FOR TRIALS ON NON-ELASTIC SOFT BODIES.

FOR this purpose the ratchets must be applied and put in order as before described, and the springs being both put to their point of rest, let the body D be put to its mark *w* in the same manner as before described, and the body C to rest. The body D being let go, and striking the body C at rest, in consequence of the stroke, the springs being hooked up by the ratchets, they both move from their resting marks *t*, *v*, respectively towards M: Now if they both moved together, and the rod *e f* covered the mark *c*, and the rod *g h* covered the mark *d* at their utmost limit, then they would truly obey the laws of non-elastic soft bodies; because their medium ascent would be to the mark *z*, which is just half the angle of ascent to the mark *x*; but as in this piece of machinery, though the main or principal springs are hooked up, yet every part of them, and all the materials of which they are composed, and to which they are attached, have a degree, or more properly speaking, a certain compass of *elasticity*, which, as such, is perfect, and no motion lost thereby.

We must not, therefore, expect the two compound bodies after the stroke to stick together without separating, as would be the case with bodies truly non-elastic and soft: but that from the elasticity they are possessed of, they will by rebounding be separated; but that elasticity being perfect, can occasion no loss of motion to the sum of the two bodies; so that if the body C ascends as much above its mark *c* as the body D falls short of its mark

mark *d*, then it will follow, that their medium ascent will still be to the mark *z*, as it ought to have been, had they been truly non-elastic soft bodies; and this, in reality, is truly the case in the experiment, as nearly as it can be discerned.

After a few vibrations, by the rubbing of the springs against one another, they are soon brought to rest; and here they would *always rest* had they been truly and properly perfect non-elastic soft bodies; but here, as in the case of these bodies, by a change of the figure and situation of the component parts, there is expended one half of the mechanic power of the first mover, yet in this case the other half is not *lost*, but *suspended* ready to be re-exerted whenever it is set at liberty; and that it is really and *bona fida one half*, and neither more nor less appears from this uncontroverted simple principle, that the power of restitution of a perfect spring is exactly equal to the power that bends it. And this may, in a certain degree, be shewn to be fact by experiment, if there were any need of such a proof; for if, when the bodies are at rest after the last experiment the two rods are lashed together near the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of scissars they will each of them rebound, C towards M and D towards N, and if they rebounded respectively to *z* and *y*, the mechanical power exerted would be the same as it was after the stroke, when the mean of their two ascents was up to the mark Z; but here it is not to be expected, because not only the motion lost by the friction of the ratchets is to be deducted, because it had the effect of real non-elasticity; but also the elasticity that separated them in the stroke, which was lost in the vibrations that succeeded; neither of which hindered the mean ascent to be to *z;* but yet, under all these disadvantages in the machine (if not unreasonably ill made) the rod *e f* will ascend to *d*, and *g h* to *a:* and hence I infer, as a positive truth, that in the collision of non-elastic soft bodies, *one half of the mechanic power residing in the striking body is lost in the stroke.*

Respecting

Respecting bodies unelastic and perfectly hard, we must infer, that since we are unavoidably led to a conclusion concerning them, which contradicts what is esteemed a truth capable of the strictest demonstration; viz. that the velocity of the center of gravity of no system of bodies can be changed by any collision betwixt one other, something must be assumed that involves a contradiction. This perfectly holds, according to all the established rules, both of perfectly elastic and perfectly non-elastic *soft* bodies; rules which must fail in the perfectly non-elastic *hard* bodies, if their velocity after the stroke is to the velocity of the striking body as one is to the square root of 2; for then the center of gravity of the two bodies will by the stroke acquire a velocity greater than the center of gravity the two bodies had before the stroke in that proportion, which is proved thus.

At the outset of the striking body, the center of gravity of the two bodies in our case will be exactly in the middle between the two; and when they meet it will have moved from their half distance to their point of contact, so the velocity of the centre of gravity before the bodies meet will be exactly one half of the velocity of the striking body; and, therefore, if the velocity of the striking body is 2, the velocity of the center of gravity of both will be one. After the stroke, as both bodies are supposed to move in contact, the velocity of the center of gravity will be the same as that of the bodies; and as their velocity is proved to be the square root of 2, the velocity of their center of gravity will be increased from 1. to the square root of 2; that is, from 1. to 1,414, &c.

The fair inference from these contradictory conclusions therefore is, that an unelastic hard body (perfectly so) is a repugnant idea, and contains in itself a contradiction; for to make it agree with the fair conclusions that may be drawn on each side, from clear premises, we shall be obliged to define its properties thus: That in the stroke of unelastic hard bodies they cannot *possibly*

*lose*

*lose any* mechanic power in the stroke; because no other impression is made than the communication of motion; and yet they *must lose* a *quantity* of mechanic power in the stroke; because, if they do not, their common center of gravity, as above shewn, will acquire an *increase* of velocity by their stroke upon each other.

In a like manner the idea of a *perpetual motion* perhaps, at first sight, may not appear to involve a contradiction in terms; but we shall be obliged to confess that it does, when, on examining its requisites for execution, we find we shall want bodies having the following properties; that when they are made to *ascend* against gravitation their absolute weight shall be *less;* and when they *descend* by gravitation (through an equal space) their absolute weight shall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea.*

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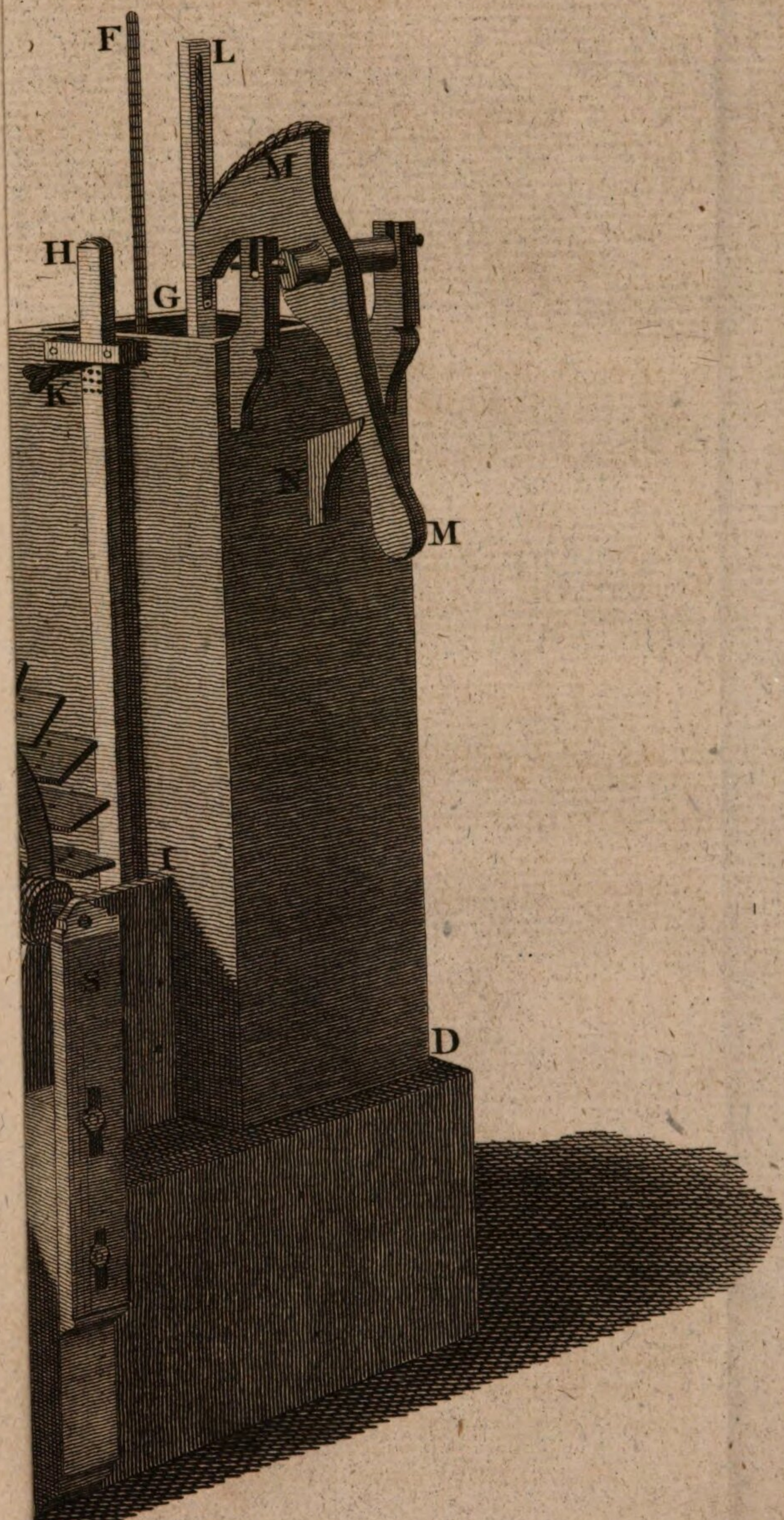


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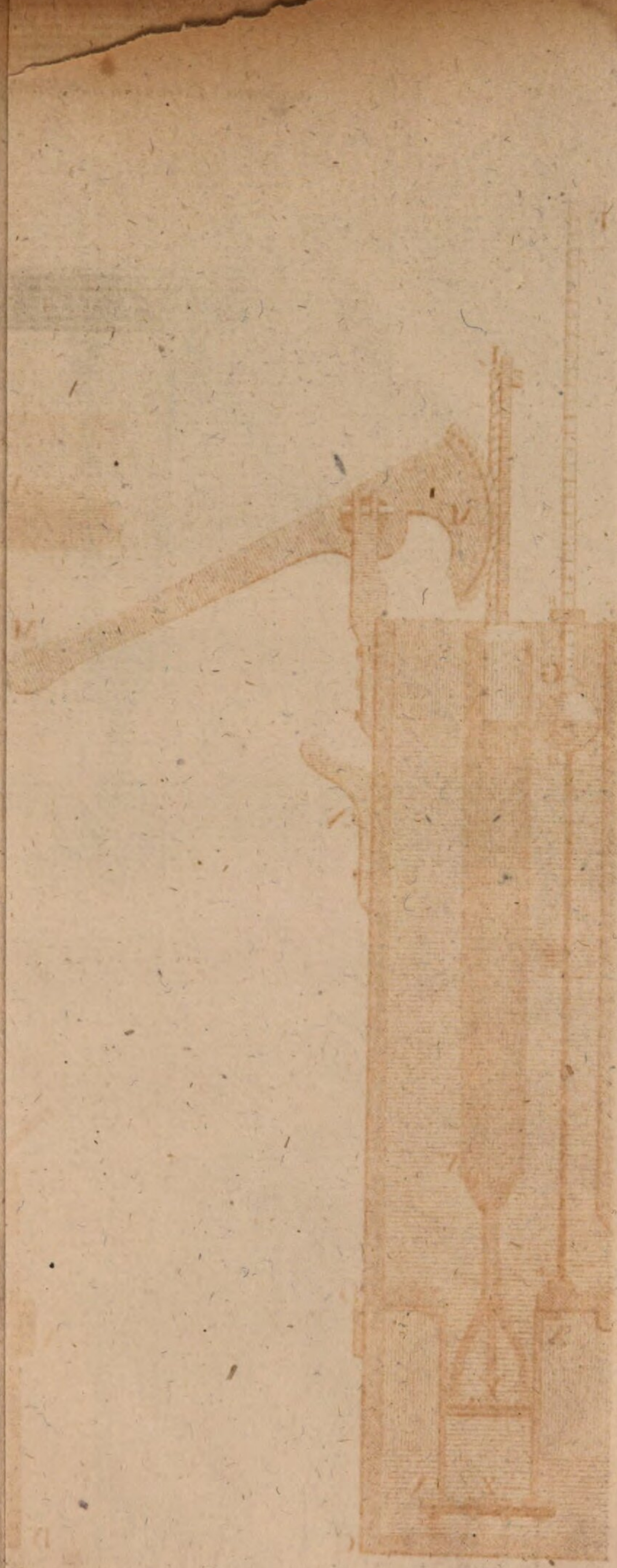

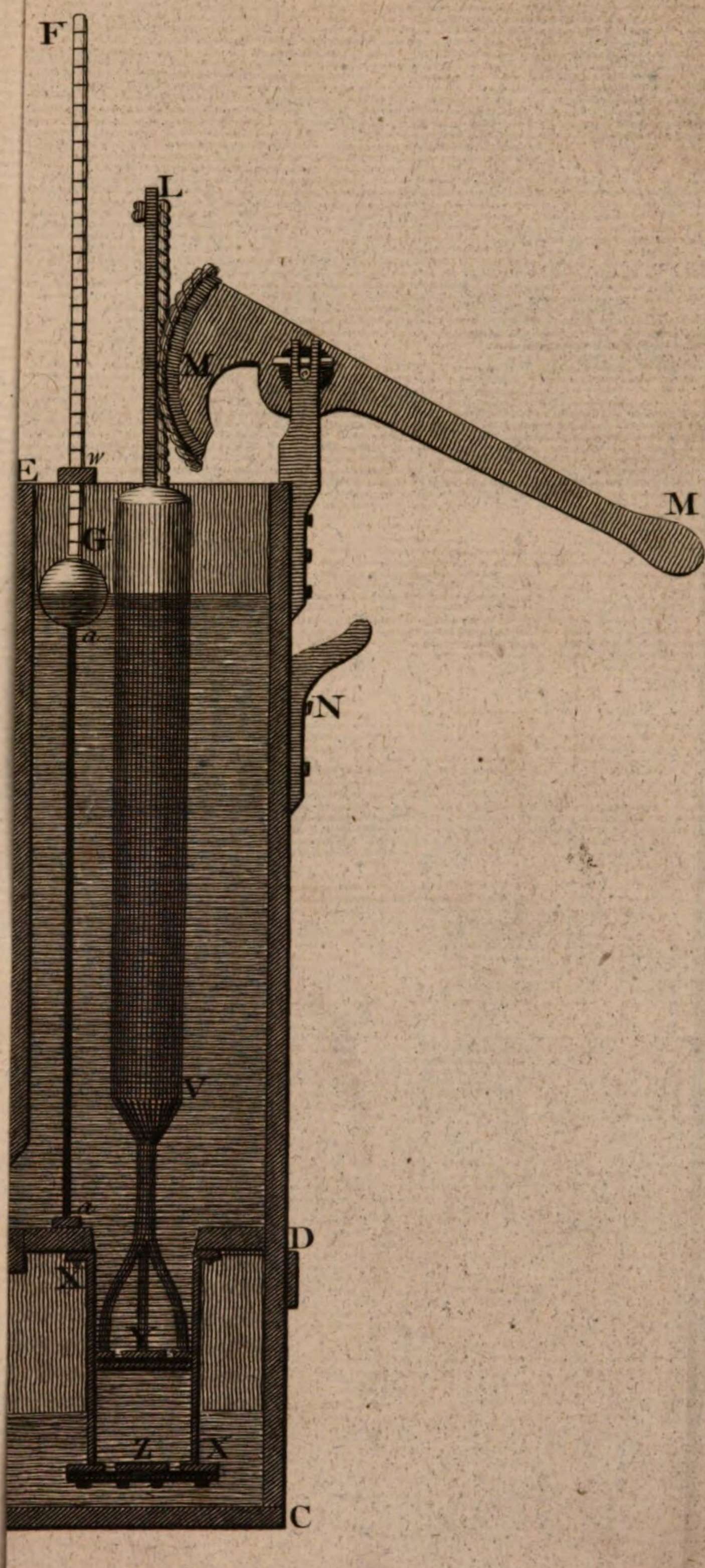


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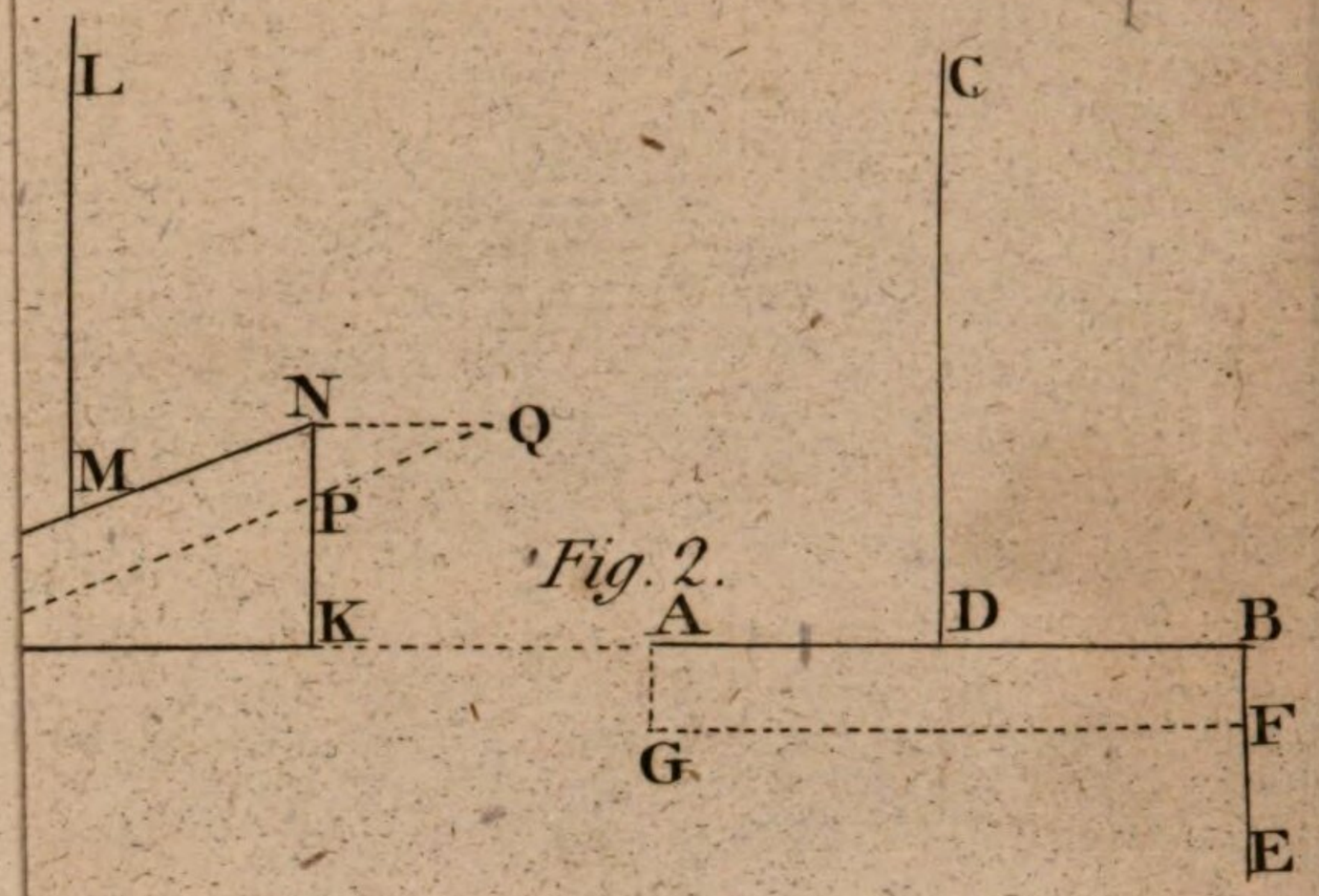


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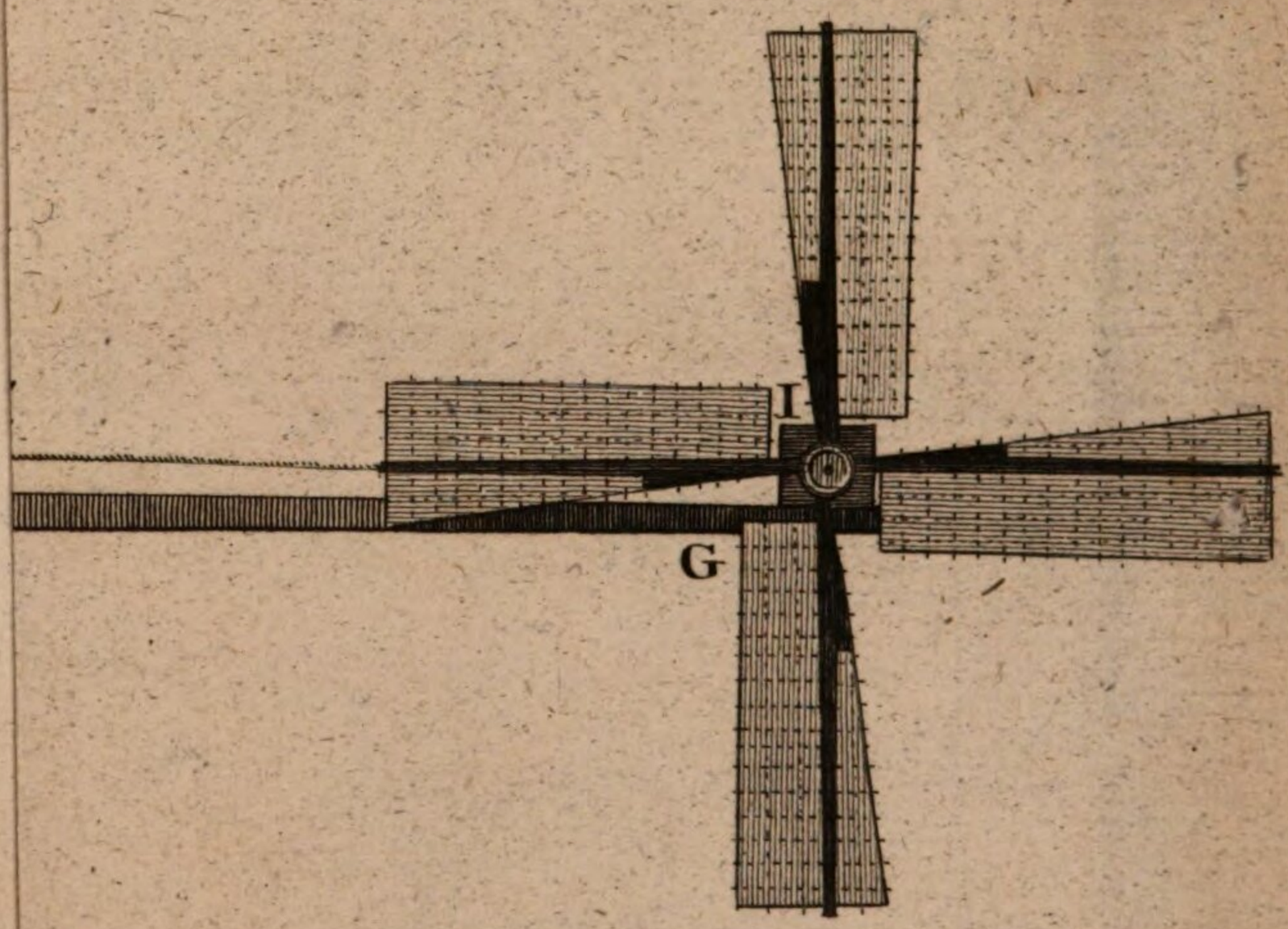

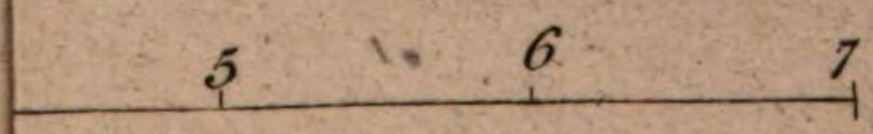


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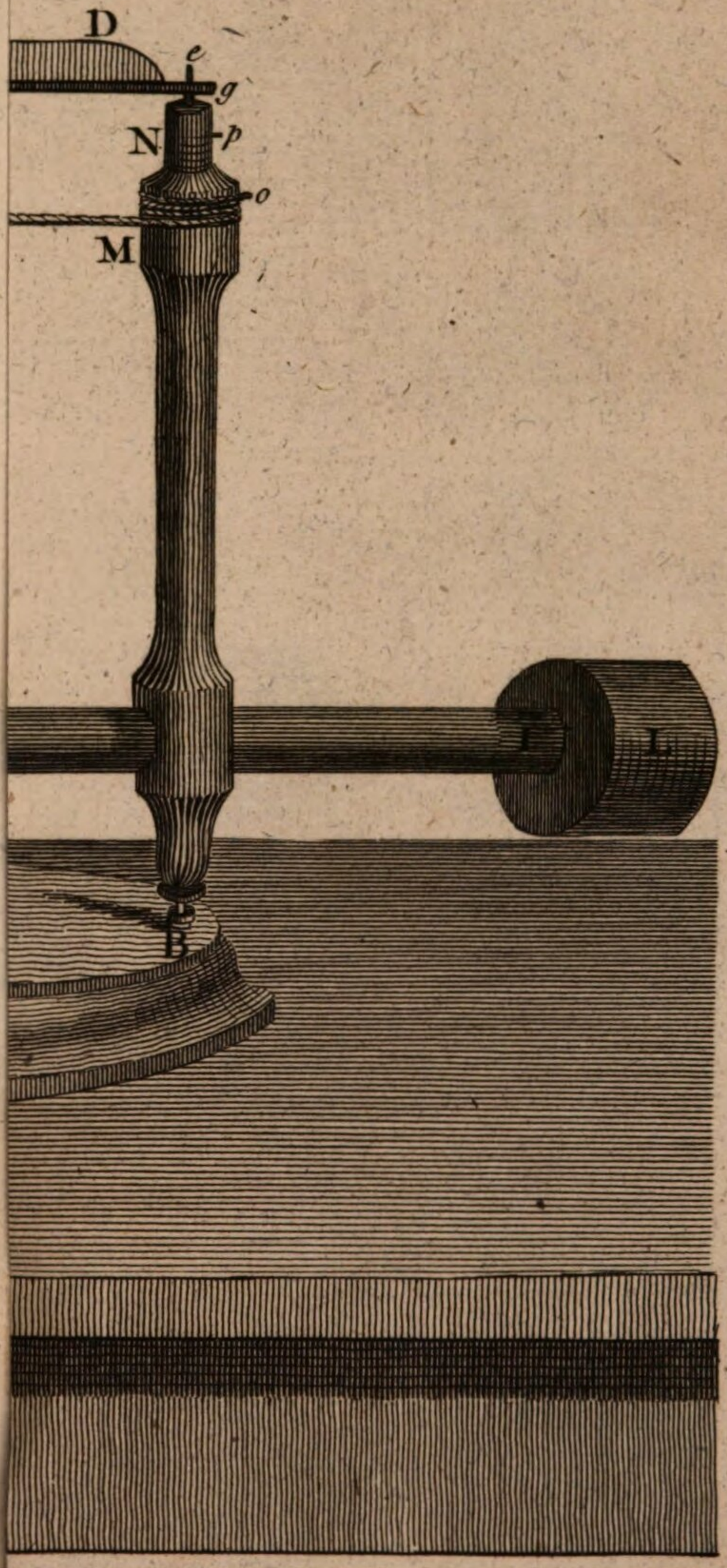


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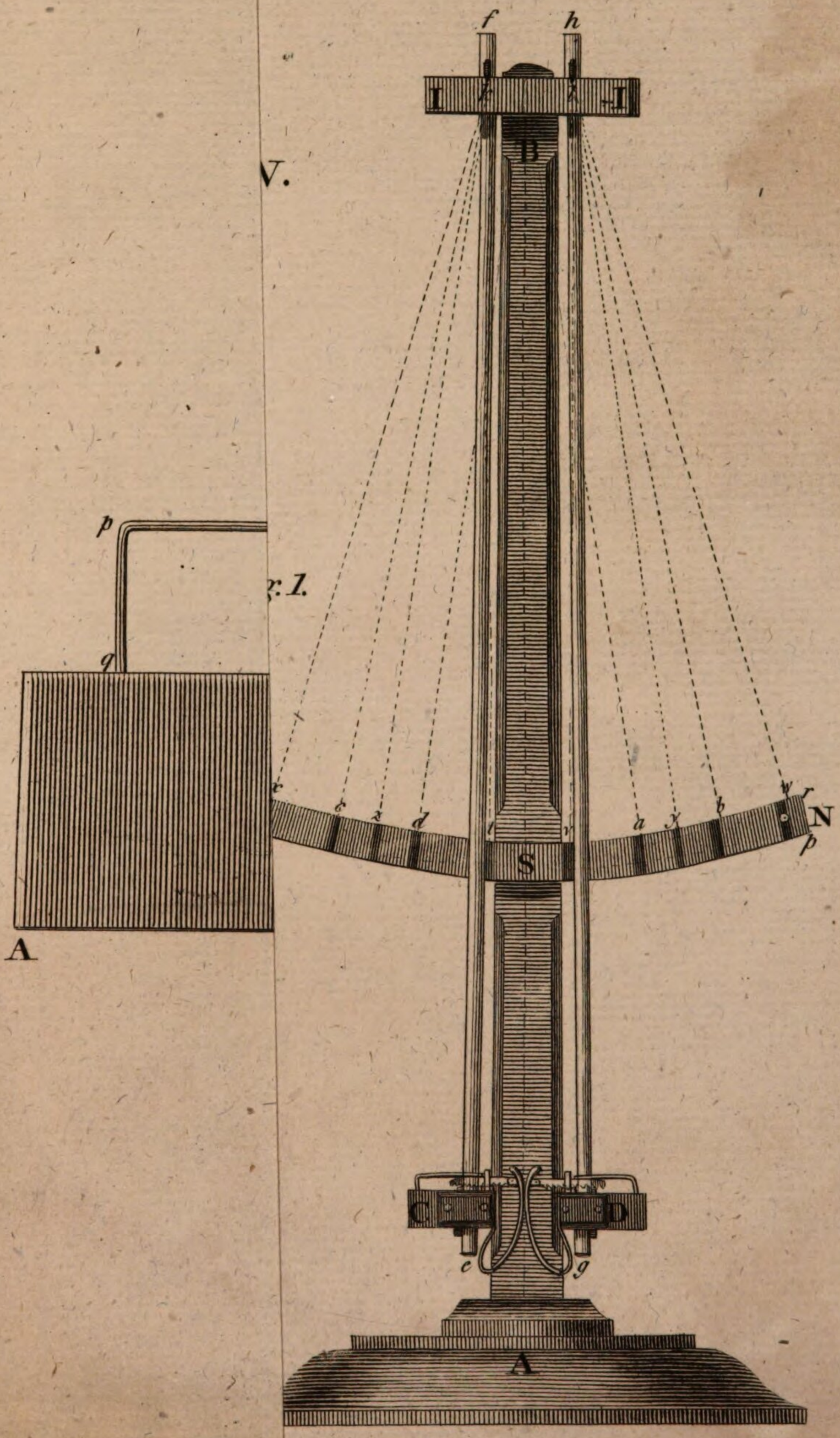


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